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ARISTOTELIAN SOCIETY

Supplementary Volume XXI.

EXPLANATION
IN HISTORY AND
PHILOSOPHY

THE SYMPOSIA READ AT THE JOINT SESSION OF
THE ARISTOTELIAN SOCIETY AND THE
MIND ASSOCIATION AT CAMBRIDGE,
JULY 4TH—6TH, 1947.

HARRISON AND SONS, LTD.
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PROGRAMME.

FRIDAY, JULY 4th.

At 5 p.m. Annual General Meeting of the Mind Association.

At 8 p.m. Chairman : Mr. M. Oakeshott.

Symposium on " Historical Explanation."

Mr. A. M. MacIver, Mr. W. H. Walsh, Professor M. Ginsberg.

SATURDAY, JULY 5th.

At 10 a.m. Chairman : Professor G. Ryle.

Symposium on " Truth by Convention."

Mr. Karl Britton, Mr. J. O. Urmson, Mr. W. C. Kneale.

At 8 p.m. Chairman : Mr. R. B. Braithwaite.

Address by Professor C. D. Broad.

" Some of the Methods of Speculative Philosophy."

SUNDAY, JULY 6th.

At 10 a.m. Chairman : Professor F. C. Bartlett.

Symposium on " Does Psychology Study Mental Acts or Mental Dispositions " ?

Mr. W. B. Gallie, Mr. W. J. H. Sprott, Professor C. A. Mace.

At 8 p.m. Chairman : Professor C. D. Broad.

Symposium on " Guilt."

Mr. H. D. Lewis, Professor J. W. Harvey, Mr. G. A. Paul.

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ADDRESS.

SOME METHODS OF SPECULATIVE PHILOSOPHY

By PROFESSOR C. D. BROAD.

IF I were asked to define "philosophy" I should begin by describing it as the kind of activity pursued by the men whom we call "philosophers" when they are engaged in their characteristic professional business. There would be no difficulty in giving instances and counter-instances of the term "philosopher." Everyone would agree, *e.g.*, that Plato, Aristotle, Descartes, Spinoza, Leibniz, Locke, Hume, Kant, Hegel, and McTaggart were eminent philosophers; and everyone would agree that Archimedes, Shakespeare, Gibbon, Gauss, and Faraday, *e.g.*, though men of the highest intellectual eminence, were not primarily philosophers. Again, there would be no difficulty in giving instances and counter-instances of philosophical activities on the part of philosophers. Everyone would agree, *e.g.*, that Leibniz was doing philosophy when he wrote his letters to Arnauld and that he was not doing it when he was writing his history of the House of Brunswick.

As in all such questions we should soon come up against the following difficulty. There would be persons about whom one would hesitate whether to call them philosophers or not, and whom some people would and others would not call by that name. And there would be activities, whether practised by admitted philosophers or admitted non-philosophers, about which there would be a similar hesitation in asserting or denying them to be philosophical. Might it not be said, *e.g.*, that Galileo, though primarily a great physicist, made important contributions to philosophy? Was Einstein merely doing mathematical physics when he enunciated first the special and then the general theory of relativity. Was he not contributing to philosophy? Lastly, one and the same person may be about equally eminent both in activities which are universally held to be philosophical and in others which are universally held not to be so. Outstanding examples are Whitehead

and Eddington in our days, and Descartes and Leibniz in former times. In such cases there will be certain marginal activities about which we hesitate to say whether they are or are not philosophical.

Now it might be suggested that the phrase "philosophical activity" is fundamentally ambiguous, *i.e.*, that an activity is called "philosophic" if and only if it has one or more of a certain limited number of alternative characteristics which neither entail nor exclude each other and which cannot be regarded as specific modifications of a single generic characteristic. It might be said, *e.g.*, that Hume and Hegel were certainly both philosophers; that Hume was undoubtedly philosophizing, well or ill, in his analysis of causation; and that Hegel was undoubtedly philosophizing, well or ill, in his attempt to prove by the dialectical method that the universe has a certain complicated kind of formal structure. But, it might be said, there is no single non-disjunctive characteristic, and no conjunction of such characteristics, common and peculiar to what Hume was doing and what Hegel was doing. To philosophize, on this view, is to perform one or another or a mixture of at least two fundamentally different kinds of activity, one of which is exemplified by Hume's attempt to analyze causal propositions and the other by Hegel's attempt to establish the formal structure of the universe by dialectical reasoning.

I think it is quite clear that the word "philosophy" has always been used to cover the kind of thing that Hegel did and that McTaggart did in addition to the kind of thing which Hume did and which Moore does, whether or not these be two radically disparate kinds of activity. Anyone who proposes that the name "philosophy" shall be confined to the latter kind of activity is proposing that it shall henceforth be used in a new and much narrower sense, and he should be expected to give reasons for this linguistic innovation. He might, *e.g.*, give as his reason that philosophizing, in the sense of doing the kind of thing that Hume did, is a practicable and useful activity; whilst philosophizing, in the sense of doing the kind of thing which Hegel did, is not only impracticable and therefore useless,

but is also a deceptive activity, based on certain fundamental illusions which have now been detected and explained but are still dangerously insidious.

This would, of course, need to be proved, and the proof would certainly be "philosophical" in some sense or other. Anyone who used it would therefore have to be careful that he did not, as Hume inadvertently did, condemn his own writings by implication to the flames, *i.e.*, that he did not employ premises or modes of reasoning which are "philosophical" in the sense which he condemns as impracticable and deceptive. He would also have to explain, if he could, how it came about that the practicable and useful activity had been so intimately associated throughout the history of philosophy with the impracticable and spurious one.

This brings me to my main point. I am inclined to think that there are two features which are together characteristic of all work that would generally be regarded as philosophical, and a third which is often present in a high degree but may be evanescent. The two which I think are always present may be called "analysis" and "synopsis"; the one which may be present in a vanishingly small degree can be called "synthesis." Analysis and synopsis themselves may be present in very different degrees and proportions. Hume's work, *e.g.*, is so predominantly analytic that it might be denied to be synoptic, and Hegel's is so predominantly synoptic that it might be denied to be analytic. But I believe that both are always present, and that each involves some degree of the other. Lastly, there is a very high positive correlation between synopsis and synthesis. Synthesis presupposes synopsis, and extensive synopsis is generally made by persons whose main interest is in synthesis.

In this paper I do not propose to say anything further about philosophical analysis. Everyone is familiar with instances of it and knows roughly what the phrase means, and the notion has been discussed *ad nauseam* in England and America during the last twenty-five years. Let it suffice to say crudely that it consists in clearing up the

meanings of all the fundamental kinds of sentence which we habitually use, *e.g.*, causal sentences, material-thing sentences, sentences with the word "I" as grammatical subject, sentences with temporal copulas, ethical sentences, religious sentences, and so on.

Synopsis and synthesis are specially characteristic of what may be called "speculative philosophy," and that is why the latter phrase occurs in the title of my paper. I will begin with the notion of synopsis.

There are different departments of fact, or different regions or levels within a single department, which it is very unusual for the plain man or even the professional scientist or scholar to contemplate together and to view in their mutual relationships. Yet they do co-exist and are relevant to each other and they must presumably be inter-related in some coherent way. Most men at most times, and many men at all times, conduct various parts of their living and their thinking in relatively watertight compartments ; turn blind eyes to awkward, abnormal, or marginal facts ; and skate successfully on the surface of phenomena. But the desire to see how the various aspects of experience hang together does arise from time to time in most intelligent men, and philosophers are persons in whom it is specially strong and persistent. Now I understand by "synopsis" the necessary preliminary towards trying to satisfy this desire, *viz.*, the deliberate viewing together of aspects of human experience which are generally viewed apart, and the endeavour to see how they are inter-related. I shall now give several examples to illustrate what I have in mind when I speak of synopsis.

Examples of Synopsis.—(1) As our first example we will take the problem of sense-perception. Why is there a problem ? (i) In the first place, because, if we attend carefully, we note such facts as these. (a) Two observers, who are said to be seeing the same part of the same thing at the same time, are often not being presented with precisely similar visual appearances of that object. (b) One and the same observer, who is said to be seeing the same unchanged part of the same thing at different times, and

from different positions, is often not presented with precisely similar visual appearances of that object on both occasions. Commonsense is, of course, more or less aware of such minor variations in normal sensible appearances, and it has certain modes of expression for describing them ; but in the main it ignores them. Certain sciences and arts, *e.g.*, geometrical optics and theory of perspective, deal explicitly and systematically with some of these facts.

(ii) Secondly, because there are visual experiences which are abnormal in various ways and degrees, but are similar to and continuous with those which are normal. They range, *e.g.*, from mirror-images and straight sticks that look bent when half immersed in water, through double images seen when one eyeball is pressed aside or when the percipient is drunk, to dreams and full-blown waking hallucinations. Those which come at the wilder end of this scale cannot plausibly be interpreted in the naively realistic way in which the language of commonsense suggests that normal sense-perceptions should be interpreted.

(iii) Thirdly, because of facts which are still quite unknown except to a minority of grown-up educated persons, and which must have been completely hidden from everyone at the time when the language in which we express our sense-experiences was first formed and for thousands of years afterwards. One of these is the physical fact that light takes time to travel ; and that the visual appearance which a remote object presents at any time to an observer depends, not on the shape, size, position, etc., of the object at that moment, but on what they were at the moment when the light now striking the observer's eye left the object. Another of them is the physiological fact that visual appearances vary with certain changes in the observer's eye, optic nerve, and brain even when the retinal stimulus is precisely similar ; and the psychological fact that they are in part conditioned by his past experiences and present expectations.

There is a problem of sense-perception, in the philosophical sense, for those and only those who try to envisage

all these fact together and to interpret sense-perception and its implications in relation to all of them. Since it is plain that they are all relevant to it, it is desirable that *someone* should take this synoptic view. Since the language in which we express our visual sense-perceptions was formed unwittingly in prehistoric times to deal in a practical way with a kind of normalized extract from our visual experiences, and in complete ignorance of a whole department of relevant physical, physiological, and psychological facts, it would be a miracle if it were theoretically adequate and if it were not positively misleading in some of its implications. And, since it is not the business of the plain man or the physicist or the physiologist or the psychologist, as such, to undertake the synopsis, it is desirable that a special group of experts with adequate factual knowledge and suitable training and interests should do so. These experts are professional philosophers.

(2) As a second example of synopsis I will take what may be very roughly called the "mind-body" problem. (i) It is plain to commonsense that many of a person's sensations and feelings follow immediately upon and vary concomitantly with certain events in his eyes, ears, joints, etc. On the other hand, many experiences, *e.g.*, processes of day-dreaming, deliberating, reasoning, etc., do not seem *prima facie* to be covariant with events in the body. Again, it is plain to commonsense that certain of a person's overt bodily movements follow immediately upon and vary concomitantly with certain of his experiences, *viz.*, his desires and intentions to express certain thoughts or to make certain changes in his own or in foreign bodies. There is no doubt at all that the popular ideas of cause and effect, and the associated ideas of agent and instrument, of activity and passivity, and so on, are mainly if not wholly derived from the facts which I have been describing.

(ii) The sciences of physiology and anatomy make it almost certain that the *immediate* bodily antecedents and correlates of sensations and feelings are *not* events in one's eyes, ears, joints, etc., but are slightly later imperceptible chemical or electrical changes in certain parts of one's

brain. They also make it almost certain that the *immediate* bodily consequents and correlates of setting oneself to fulfil an intention are *not* the overt bodily movements which one is setting oneself to make, but are slightly earlier imperceptible chemical or electrical changes in certain parts of one's brain.

(iii) It is further alleged, on the authority of these sciences, that there are immediate bodily antecedents and correlates of the same general nature, viz., chemical or electrical events in certain parts of the brain, even to those mental processes, such as deliberating, comparing, abstracting, reasoning, etc., which do not seem *prima facie* to be covariant with bodily events.

(iv) The physical sciences have developed a concept of causation in terms of regular sequence and concomitant variation, in which the notions of agent and instrument, activity and passivity, etc., play little if any explicit part. So far as the notion of acting and being acted upon survives in physics, the physicist thinks of one system as acting on another when energy is transferred from the former to the latter ; or when the former, without doing work on the latter, modifies the direction of motion of some of its parts by fixed constraints. Now there is fairly good empirical evidence that a living organism never gains nor loses energy except by transference from or to some other part of the *material* world. And it is difficult to picture a volition exercising guidance without work on atoms or electrons in the brain, as a material constraint, such as the thread of a pendulum, does on a moving macroscopic body, such as a pendulum-bob.

Now these various mutually relevant facts are hardly ever viewed synoptically except by philosophers. Common-sense is quite ignorant of many of them and common language had grown up and crystallized ages before they were known or suspected. On the other hand, scientists who are familiar with all of them tend to concentrate on one at a time and temporarily to ignore the rest. When they confine their attention to the physical and physiological and anatomical facts they are inclined to take the

view that men are "conscious automata," *i.e.*, that all our mental states, including processes of reasoning, willing, etc., are mere by-products of states of brain which are determined by purely physical and physiological antecedents. But their daily lives and all their professional activities presuppose a view which is shared by plain men and which seems *prima facie* to be incompatible with the conscious automaton theory.

Scientists all assume in practice that, when they design and carry out an experiment, they are initiating certain changes in the material world which would never have taken place unless they had been thought out beforehand, desired, and deliberately led up to. They assume that their assent to or dissent from the various alternative interpretations which might be put on the results of an experiment is determined by processes of *reasoning*, demonstrative or probable, in which belief is given or withheld in accordance with *evidence*, which may be favourable or unfavourable, weak or strong or coercive. Now all this involves concepts, and seems *prima facie* to involve modes of causation, completely different from those in terms of which the conscious automaton theory is formulated.

To sum this up briefly. The scientist who investigates and theorizes about man and his powers and activities is himself a man exercising certain characteristically human powers and activities. But the account which he is apt to give of man, when he treats him as an object of scientific investigation, seems *prima facie* difficult to reconcile with the occurrence and the validity of his own most characteristic activities as investigator, experimenter, theorist, and reasoner. The need for synopsis by someone who is aware of all the main facts and can hold them steadily together in one view is here particularly obvious.

(3) As a third example of synopsis I will take what may roughly be called the "free-will" problem. The main facts are these. (i) Suppose I believe that a certain course of Action *A* is right, or that it would be to my interest in the long run ; and suppose I know that it is difficult and repellent in itself or in some of its consequences, and

that I shall carry it through only if I make a continuous and exhausting effort. Suppose I believe that alternative *B* is wrong or is against my long-term interests, but know that it requires no particular effort and that I shall certainly slip into it if at any time I cease to put forth effort in the direction of *A*. Suppose that I decide on *A* and set myself to carry it out. It may happen after a while that, in full consciousness that *A* is right or is to my long-term interest and that *B* is wrong or contrary to that interest, I deliberately cease to put forth the required effort in the direction of *A* and, as we say, "let things rip," and slip into *B*. Or it may be that, before doing this, I deliberately banish to the margin of my consciousness my knowledge of the reasons for *A* and the reasons against *B*, and let the reversion from *A* to *B* take place in the temporary and superficial state of half-deception which I have deliberately created. In either case, it seems to me, I am quite convinced that I could have done otherwise. I could have kept up the effort, or made the required increase in it, in the *A*-direction. I could have kept the relative merits of *A* and *B* fairly together before my mind. Moreover, "could" seems to be used here in some sense which is not reducible to "would have, if." I do not seem merely to mean the triviality that I should have acted differently if I had willed differently, or that I should have willed differently then if I had willed differently in the remoter past, or that a person with a different nature and dispositions from mine would have acted differently.

(ii) Many of our common moral judgments and morally directed emotions, *e.g.*, the judgment that a person ought to have done something which he did not do, and the feeling of remorse for something which one has done and believes to be wrong, seem to presuppose the truth of this conviction. All such judgments are false *in principle*, and all such emotions are *in principle* misplaced, unless it be true that the very same person who in fact willed *X* and put forth a certain degree of effort to realize it could at that moment and in those circumstances have instead willed *Y* or put forth a different degree of effort to realize *X*.

(iii) Many people find it self-evident on reflexion that, given the dispositional properties of a substance, its past history, its circumstances at a given moment, and the laws of nature (including those of psychology), it is impossible that anything should then have happened in it other than what did happen.

(iv) Whether this be self-evident or not, everyone does in fact assume it or something like it in all his practical and theoretical dealings with macroscopic physical events not due to volitions and with mental events other than volitions. In particular, physiologists assume that those events in one's brain which they take to be the material basis of our volitions and our puttings-forth of effort, are completely determined by physical causes. If so, how can the volitions and the puttings-forth of effort, which are held to be the mental aspect or the product of these events in our brains, be in any sense or degree undetermined?

(v) Whilst it is difficult to reconcile the notion of moral responsibility, merit and demerit, etc., with the view that our volitions and our puttings-forth of effort are completely determined, and particularly with the view that they are completely determined by physical causes, the mere admission that they are within certain limits undetermined would not suffice to give what is wanted. For it is required that they shall be expressions of our permanent inner nature and not merely accidental events that happen in us. And yet, just in so far as they are undetermined, they seem to fall under the latter heading.

Here again the need for synopsis is evident. It seems *prima facie* that each of us conducts one part of his life on the assumption of complete determinism and another part on the assumption of incomplete determinism *plus* something else more positive which it is very hard to formulate clearly. And these two parts are not sharply separated; they overlap and interpenetrate each other. Most of us generally manage to ignore one aspect at a time and concentrate on the other; but, however convenient this may be in practice, it is intolerable in theory to anyone with a tidy mind who has become aware of the facts.

(4) The examples which I have given have been taken from fields which philosophers have long and diligently tilled. My fourth and last example will be taken from a region which most of them still disgracefully neglect to familiarize themselves with or to cultivate. I allude to those facts which have been alleged and suspected throughout the ages and have for the first time been properly investigated and in part verified during the last sixty years by the Society for Psychical Research in England and by other workers in the U.S.A. and on the Continent. I shall refer to these facts as "paranormal phenomena." The following are, in my opinion and in that of most persons who have given adequate time and trouble to the study of the relevant evidence, well established.

(i) The following forms of paranormal cognition have been established under rigidly controlled experimental conditions. (a) A subject may cognize correctly, with a frequency which so greatly exceeds chance expectation that the odds against such an excess being fortuitous are astronomical, what another person has been and is no longer perceiving, under conditions where there is no possibility of relevant information being conveyed to him by normal sensory means. This may be described as "post-cognitive telepathy." The same is true if we substitute the phrase "is contemporaneously cognizing" for "has been and is no longer cognizing" in the above sentence. This may be called "simultaneous telepathy." (b) A subject may cognize correctly, with a frequency which exceeds chance-expectation to the same high degree, what he himself or another person will begin to perceive at some later date, under conditions where there is no possibility of his consciously or unconsciously inferring this future event either with certainty or with probability from any data available to him at the time. The two cases here described may be called respectively "precognitive auto-scopsy" and "precognitive telepathy."

(ii) There is a mass of well-attested and carefully investigated cases of the following kind. A has an hallucinatory waking experience of a very specific and uncommon

kind, and this experience either imitates in detail or unmistakably symbolizes some crisis in the life of a certain other person *B*, e.g., death or a serious accident or sudden illness, which happens at roughly the same time. Such sporadic correlated experiences may happen when *A* and *B* are separated by great distances, and where *A* had no reason whatever to expect that any such event would happen to *B*. Any one of them taken separately might perhaps be regarded as an extraordinary chance-coincidence. But I do not think that anyone who is aware of the number and variety of such cases which have stood up to critical investigation could possibly regard this as a reasonable account of them taken collectively. It is plain that, on any ordinary criterion of causation, there is some causal connexion between the occurrence of the crisis in *B* and the occurrence at about the same time in *A* of the waking hallucination which imitates it or symbolizes it.

(iii) Cases of the following kind have been observed and very carefully investigated. Each of a number of different persons, *A*, *B*, *C*, and *D*, living in remote places and not communicating with each other, is producing a series of automatic scripts at frequent intervals throughout the same longish period. A certain peculiar phrase or sentence, and subsequent repetitions and variations of it, will begin to appear in *A*'s scripts. This will have no special significance for *A*. A certain different phrase or sentence, and subsequent repetitions and variations of it, will begin to appear at about the same time in *B*'s scripts. This will have no special significance for *B*. The same will be true *mutatis mutandis* for *C* and *D*. When the scripts of *A*, *B*, *C*, and *D* are compared, it is found that these separately insignificant phrases or sentences combine to indicate unmistakably a certain out-of-the-way classical, literary, or historical topic. This is generally quite outside the normal knowledge of most of the automatists. On the other hand, it *was* within the knowledge of a certain deceased scholar, e.g., F. W. H. Myers, Dr. Verrall, or Professor Butcher ; it had a special interest and significance for that scholar when he was alive ; and the scripts claim to be communications from

him. Moreover, in the context of such phrases or sentences in *A*'s scripts there will often occur a request that reference shall be made to the contemporary or subsequent scripts of the other automatists for further elucidation of the meaning ; and a statement will be made that the communicator is trying to convey an idea by this roundabout method as an experiment. Such cases are called " cross-correspondences," and there was an outbreak of them among certain automatists soon after the deaths of Myers, of Verrall, and of Butcher. They clearly involve intelligence and intention of a fairly high order on the part of *some* person or persons, living or dead, and the power to carry out that intention by telepathically influencing a number of minds in different but appropriately interconnected ways. And they certainly suggest *prima facie* the continued existence, intellectual activity, and planned action of certain definite individuals after the death of their bodies.

Now the philosophical importance of the paranormal facts which I have been describing is due to the following circumstances. They are not merely odd and uncommon, like the feats of calculating boys or the occurrence of babies with six toes or with webbed fingers. They seem to conflict with certain very general restrictive principles which we unhesitatingly take for granted as the fixed framework within which all our practical life and our scientific theories are confined. We assume, *e.g.*, that the only ways in which one person can possibly get to know about the contemporary or past experiences of another are the following. Either by hearing and understanding sentences which he utters or reading and understanding sentences which he has written ; or by hearing and interpreting cries which he makes or seeing and interpreting his gestures or facial expressions ; or by seeing and making conscious or unconscious inferences from persistent material records, such as pictures, tools, pottery, etc., which he has made or used in the past. Now the occurrence of post-cognitive and simultaneous telepathy, whether of the experimental or the sporadic kind, seems to involve a breach of that restrictive principle.

Again, we assume that there are only two ways in which a person can forecast a future experience of himself or of another person. One is by explicit inference from data supplied to him by his present sense-perceptions, introspections, and memories, together with his knowledge of the laws of nature. The other is by non-inferential expectations, based either on the fact that he has formed a certain intention or on associations which have been formed by certain repeated sequences in his past experience and are now stimulated by some present experience. The occurrence of precognitive autoscopy and precognitive telepathy seems to involve a breach of this restrictive principle.

Then, again, we assume almost as a self-evident principle that an event cannot begin to have effects until it has happened. This entails that it cannot contribute to cause any event that preceded it. But in the case of precognitive autoscopy or precognitive telepathy it looks as if the later event, which is foreshadowed by the earlier experience, must have been an essential factor in causing that experience. This is, I think, *one* reason, and perhaps the only *good* reason, why we are all inclined to put up a specially strong resistance against evidence in favour of paranormal precognition.

Lastly, educated commonsense in Europe and America takes for granted that, when a person's body dies, he either ceases to exist altogether, or, if not, he ceases to be able to influence ordinary physical objects or the minds of men and animals whose bodies are still living. Also, as we have seen, it is often held that science shows that human beings are conscious automata. This seems to imply that the supposition that a person's mind might survive the death of his body is either meaningless or quite certainly false. Yet the cross-correspondences and certain other mediumistic phenomena do very strongly suggest that this sometimes happens.

Now the vast mass of normal phenomena, which have led to the implicit acceptance of the restrictive principles just mentioned, coexist with the occasional paranormal phenomena which seem to conflict with those principles.

No one can ignore the former. Those who ignore the latter, and continue to philosophize as if these restrictive principles were unquestioned and unquestionable, are simply emulating that not very intelligent bird the ostrich. Those philosophers who assert that the meaning of a proposition is essentially bound up with the ways in which it might be verified or refuted or confirmed or weakened almost always tacitly assume that the only possible ways in which this could happen are normal sense-perception or introspection. Their more restrictive conclusions follow, not from the verification principle alone, but from the combination of it with this restrictive assumption about the possible forms of human cognition of particulars. Now the restrictive assumption is logically independent of the principle, and the facts which I have indicated show that it is false.

Again, if a philosopher says that such a sentence as "Mr. Jones survived the death of his body and remained intelligent and active afterwards" is meaningless, the appropriate comment would seem to be: "Well, and what then?" Obviously our ordinary modes of speech arose in connexion with the situations with which mankind has to deal in its normal daily life and not in connexion with those extremely odd and comparatively rare situations which form the subject of psychical research. There would, therefore, be nothing in the least surprising if these modes of speech should suggest mutually inconsistent ideas when they are used to describe and interpret paranormal facts. The remedy is either to invent new and more appropriate verbal forms or to stretch the meanings and implications of the old ones; it is not honest to ignore or deny the facts, and it is not helpful to stand for ever dithering and nagging about the current usage of words. The need to contemplate normal and paranormal facts in a single synoptic view is surely obvious; and it is the business of the philosopher, rather than the natural scientist, the psychical researcher, or the plain man, to try to make such a synopsis.

I hope that the four examples which I have taken will have made plain what I mean by "synopsis"; why I

think that it needs doing ; and why it seems desirable that persons with the special kind of training and interests which we call "philosophic" should undertake this task.

Synopsis and Analysis.—I think that there is a very close connexion between synopsis and the process of analysis which everyone admits to be a characteristically philosophical activity. It is generally synopsis which gives the stimulus to analysis. As I have shown in my examples, it often happens that each of several regions of fact, which we generally contemplate or react to separately, gives rise to its own set of concepts and principles ; that each such set seems satisfactory and internally coherent ; but that, when we contemplate these various departments together, we find that the corresponding sets of concepts and principles seem to conflict with each other. The intellectual discomfort thus produced in a person of philosophical disposition is perhaps the most usual motive for trying to analyze those concepts and to formulate those principles clearly. Such a process is an indispensable step towards deciding whether the inconsistency is real or only apparent and towards formulating it precisely if it is real ; and this is a precondition of any efficient attempt to resolve it.

Synopsis and Synthesis.—Synopsis is not an end in itself. It not only provides the stimulus for analysis, but it also furnishes the basis for something else, which may be called "Synthesis." The purpose of synthesis is to supply a set of concepts and principles which shall cover satisfactorily all the various regions of fact which are being viewed synoptically. The concepts and principles characteristic of each separate department, in so far as they are valid, must be shown to follow from, or at least to cohere closely with, this more general set, under the special conditions and limitations peculiar to that department. In so far as any of them are not strictly valid it must be shown why they are so nearly so that they seem to be completely satisfactory while we confine our attention to that department. The apparent conflict between the concepts and principles characteristic of different regions of fact must be shown to arise from the valid application of these common

concepts and principles in different contexts and under different special limitations. Even when there is no conflict to be solved it is likely that contemplating together several regions of fact, which are usually contemplated and reacted to separately, will reveal certain analogies between their contents or their structure and certain inter-relations between them as collective wholes.

Some further Remarks on Synopsis and Synthesis.—Having now explained the notions of Synopsis and Synthesis and their inter-relations, I want to make some supplementary remarks about them.

(1) Intellectual activities which are genuinely philosophical, in that they involve deep analysis, wide synopsis, and illuminating synthesis, occur from time to time within some special science. This is particularly obvious when the science is concerned, as physics is, with very fundamental and pervasive features of reality. I could certainly count as philosophical the work done by Galileo on the analysis of kinematic and dynamical phenomena, and the correlated work of synthesis in which the formulation of the three laws of motion and the law of gravitation by Newton is an outstanding phase and the unification of these laws by Lagrange, Hamilton, and finally Einstein is a further development.

Again, the situations which led respectively to the formulation of the Principle of Relativity and the Uncertainty Principle are typical of what I have exemplified under the head of synopsis, and the principles themselves are typical of what I have described as synthesis. In the case of relativity there were many different kinds of possible experiments which, in accordance with well-tried and generally accepted principles, might have been expected to provide perceptible evidence for the motion of a body relative to the surrounding ether. The results of all these experiments were completely negative. Yet, on the other hand, there were also many known facts which, in accordance with well-tried and accepted principles, were incompatible with the supposition that a moving body drags the surrounding ether along with it. In the case of quantum

mechanics there was an enormous mass of very accurately known and highly co-ordinated facts which seemed to entail that light is of the nature of transmitted waves and not of the nature of emitted particles, and there were other facts, just as accurately known and as highly co-ordinated, which seemed to entail the exact opposite. The Principle of Relativity and the Uncertainty Principle are clear instances of synthesis, based on synopsis, and preceded and made possible by a more profound analysis of generally accepted concepts and principles.

The results of such synthesis in physics have the advantage that either they themselves can be stated mathematically or that they impose certain conditions on the form of equations which express possible physical laws. Hence their consequences can be rigidly deduced. This is seldom, if ever, true of syntheses which cover several widely different fields of fact, *e.g.*, man considered as reasoner, experimenter, and morally responsible agent, and man considered as an object of physiological and psychological experiment.

It is worth while to remark that a certain type of synthesis may be suggested by a great philosopher, who is not an expert physicist or mathematician, or at a time when physics and mathematics had not reached a certain stage of development which it afterwards attained. His suggestion may then and for long afterwards be difficult to grasp and may seem very unplausible. Yet, when physics and mathematics have developed further, it may be easy for an expert to formulate it clearly and to work out its consequences and for non-experts to grasp it. And it may then be seen to be quite plausible and very illuminating. I can think of several examples, but I shall content myself with the following.

In the Second Book of his *Ethics* Spinoza tries to formulate a theory of bodies consistent with his general principle that there are no finite continuants, that the only genuine continuant is God, and that God is a substance which is at once material and mental. I doubt if it was possible for Spinoza or anyone else to formulate such a theory of bodies

clearly or satisfactorily at the time. But in the XIXth century Lord Kelvin was able to state clearly and to work out in considerable detail the theory that an atom is a state of persistent localized vortex-motion in the ether ; that what we count as changes in an atom are of the nature of perturbations in such a persistent localized circulation ; and that compounds are related to their elements somewhat in the way in which a complicated wave-system is analyzable into superposed simple-harmonic component waves. If we think of Spinoza's "God considered under the Attribute of Extension" as equivalent to Lord Kelvin's ether and Spinoza's *corpora simplicissima* as equivalent to Lord Kelvin's vortex-atoms, Spinoza's suggestion, at any rate as regards the material aspect of reality, becomes intelligible and illuminating.

(2) Synopsis and synthesis both take place at various levels. I have just given examples of them within a single region of fact, viz., that of physics. At a higher level one would try to get a synoptic view, *e.g.*, of the phenomena of organic and inorganic material things and processes, and try to synthesize them into a single coherent scheme. At a still higher level one would take into one's view the facts of mental life at the animal level, and then at the level of rational cognition, deliberate action, specifically moral emotion and motivation, and so on. Finally, if no account had so far been taken of paranormal phenomena, these would have to be brought into the picture, and an attempt made to synthesize them with the normal facts. As each new department was considered it would be necessary to review the syntheses which had seemed fairly satisfactory at the previous level. Some of them might not need to be rejected or even seriously modified, but others might have to be completely abandoned or considerably altered when a new department of facts was brought into the picture.

Here again I will give one example out of several which I could mention. Bergson suggested the theory that the function of the brain and nervous system and sense-organs is in the main eliminative and not productive. According to him, each person at each moment is potentially capable

of remembering all that has ever happened to him and of perceiving everything that is happening anywhere in the universe. What has to be explained is, not how we *do* remember or perceive the particular events and things of which we are consciously aware at any moment, but why we *do not* then remember or perceive any events or things *beside* these. According to Bergson, the function of the brain and nervous system is to protect us from being confused and overwhelmed, to shut out enormously the greater part of what we should otherwise perceive and remember at any moment, and to leave us only with that very small and very special selection from our knowledge which will be biologically useful at the time.

Now it is true that Bergson enunciates and defends this theory in reference to its alleged close coherence with the facts of normal cognition and its pathological disturbances. But my impression is that, so long as we confine ourselves to that region, the suggestion, though ingenious and original, is hardly plausible. It seems to me, however, to take on a very different aspect when we bring the facts of paranormal cognition into the picture. Many of them seem to fit very well into this part of Bergson's scheme and rather ill into the more usual view of the function of the brain and nervous system in cognition. Now, if that be so, it may be necessary to revise certain of one's previous attempts at synthesis. It may behove us to try much more seriously to synthesize the facts of normal cognition on Bergsonian lines.

How are Principles of Synthesis Discovered?—I am sure that it is impossible to give rules for the discovery of principles of synthesis in philosophy, just as it is impossible to give rules for suggesting fruitful hypotheses and colligating a mass of observations in science. But the following remarks on the general procedure of speculative philosophers may be worth making.

(1) What often happens is this. A philosopher is strongly impressed by some feature which is highly characteristic of a certain important region of fact, and which within that region is felt to be completely intelligible and a source

of satisfactory explanations. He then discovers or thinks he discovers analogies between that region and others in which that feature is at first sight not prominent and perhaps not even noticeable. Then he may note that, even within the region of which this feature is characteristic, it appears in a whole range of different forms and different degrees. In some of these it stares one in the face ; in others, it might have escaped notice altogether unless they had been connected with the outstanding instances by a series of intermediate cases. He then tries to abstract and generalize this feature into a flexible principle, capable of manifesting itself in very dissimilar ways in different regions of fact, and such that the differences in its manifestations are connected in an intelligible way with differences in the circumstances. Finally, he tries to show that this principle is, in fact, operative in those regions in which it seemed at first sight not to be so. In this way, he feels that he has discovered order and unity pervading the collection of various regions of fact which he is surveying synoptically.

As an example of this I will take Aristotle's concept of Matter and Form. This seems to have been a generalization from the very familiar fact of a workman or artist making out of a common mass of raw material, *e.g.*, clay, a number of artificial objects of various kinds, *e.g.*, cups, plates, bricks, etc., in accordance with an idea of such an object and a desire for it which is already present in his mind and guides his actions in making it. There is the further fact that the informed matter resulting from one such operation, *e.g.*, bricks, may become the raw material for another such operation of a higher order, *e.g.*, the building of a house. Now this notion covers a very large region of human activities and their products, and within that region provides perfectly satisfactory accounts of the origin of particular objects. Again, there are many other human activities which present obvious analogies to that of deliberately making an artifact out of raw materials, and yet diverge from it in various important directions, *e.g.*, making a speech, writing an essay, singing a song, and composing and playing a bit of music. Lastly, there are

certain products of animal activity, *e.g.*, nests and honeycombs, which look very much like human artifacts.

Now the development, self-maintenance, and reproduction of a plant or animal obviously resembles the deliberate production of an artificial object in certain respects, and equally obviously differs *prima facie* in others. These processes look as if someone had the desire to produce and maintain, *e.g.*, an oak-tree or a cat ; as if he tried and for a while succeeded in imposing the oak-form or the cat-form on such raw materials as water, carbon-dioxide, mineral salts, dead mice, milk, catsmeat, etc. ; and as if sooner or later, in the case of each individual oak or cat, his efforts became less and less successful and finally failed altogether. It also looks as if someone, who foresaw this breakdown in the case of each individual, desired that there should always be oak-trees and cats, and arranged with great ingenuity that this should be secured by sexual reproduction. On the other hand, we know of no external artificer of whom we can say that he, in fact, constructed and is trying to maintain this cat or that oak-tree in accordance with an idea and a desire in his mind.

So, if we want to carry the analogy into the realm of organisms, we may be forced to do one of two things. One is to think of an individual plant or animal as an artifact produced and maintained by some *non-natural* external artificer, *e.g.*, a god. The other is to think of it as standing to *itself* in the relation of artificer to artifact, or to think of a *certain part of it*, *e.g.*, an animal or vegetable soul, as standing in that relation to the rest of it. The former development keeps the notion of an artificer who is *external* to his artifact, but at the cost of putting him outside the order of nature. The other, at the first move at any rate, keeps him within the order of nature ; but at the cost of replacing the familiar notion of a person making a thing according to his designs with the unfamiliar and barely intelligible notion of a person or a thing making *itself*, or of one part of a thing making the *rest of it*, in accordance with his or its designs.

(2) I think that a very important exercise for the speculative philosopher is to devote a good deal of attention to marginal and abnormal cases within normal and familiar regions of phenomena. The case may be put generally as follows.

It may happen that several characteristics, C_1 , C_2 , etc., are very strongly associated and have a very high positive correlation with each other in all normal human experience. The former means that it is very unusual for any of them to occur without all the rest. The latter means that a high value of any one of them is nearly always accompanied by high values of the others, and that a low value of any of them is nearly always accompanied by low values of the others. In such cases it may be difficult to distinguish the characteristics, and almost impossible to conceive that they do not entail each other or that they are not just different aspects of a single characteristic. But, if attention is paid to marginal, abnormal, or pathological cases, it may be found that some or all of these characteristics can occur in isolation from the rest, or that some can occur in high values accompanied by the others in low values. It may then strike a philosopher that their high association and high correlation in normal cases within a certain region of fact may be due to the fulfilment there of certain assignable conditions which need not be fulfilled always and everywhere. He may then be able to detect, in other regions of fact, the presence of some of these characteristics in isolation from the rest, or the presence of a combination of all of them in which some are present in a high degree and the rest in a very low degree. In this way a principle of synthesis may be suggested to him which he would not otherwise have thought of.

An obvious and elementary example of this is the importance of such experiences as mirror-images, dreams, and waking hallucinations for the philosophy of sense-perception and the physical world. In mirror-images the normal correlation between the deliveries of sight and of touch breaks down. In dreams we have visual experiences very much like those of normal waking life, but it is evident

that they occur without the eye being stimulated by light from an external object as in normal visual perception. In waking hallucinations the subject's eyes are open and he may have auditory and even tactual experiences which seem to bear out his visual experience ; but the normal correlation between what he ostensibly perceives and what other persons in his neighbourhood ostensibly perceive breaks down. Even this breakdown may be only partial, for there are well-attested cases of collective hallucination. Thus we have a series of experiences from ordinary dreams at one end, through singular and collective waking hallucinations and optical delusions, to perfectly normal waking sense-perceptions. The contemplation of such a series is philosophically most illuminating.

Another important example is provided by the study of alternating personality, co-conscious personalities, hypnosis, psycho-analysis, etc., If we confine our attention to normal grown-up Western Europeans in their most alert and integrated moments, to the *façade* which they present to their fellows in ordinary social intercourse, and to the appearance which each presents to himself when he is not taken off his guard, we shall be inclined to think it evident that every experience belongs to one and only one self, that every human organism is animated by one and only one self from the cradle to the grave, that every self animates one and only one organism, and so on. A study of the abnormal facts which I have mentioned sets us free for speculations which we should not otherwise have thought of, or should not have thought it worth while to follow up. It becomes conceivable that there may be experiences which do not belong to any self, experiences which belong to several selves, groups of experiences interconnected on other principles than those which are characteristic of selves, human organisms which are animated simultaneously or successively by several selves, and selves which habitually and predominantly animate one organism but occasionally and partially animate another, and so on. It is hardly necessary to point out how useful this shaking loose of our

associated ideas may be in trying to synthesize, *e.g.*, para-normal phenomena with normal ones.

Before leaving this topic, I would like to make the following remarks. (i) The study of mathematics has been for many persons a most important means of training the mind to separate ideas which are closely associated in experience, and to generalize ideas which are presented to us in experience only in a single determinate form. One important example is the study of non-Euclidean geometry, in which we learn for the first time to break down the association between the notions of equidistance and non-intersection in the case of two co-planar straight lines, by seeing that there are internally consistent systems of geometry in which the latter property occurs without the former. Another very important example is the generalization of the sense-given three-dimensional spatial order to the notion of a manifold of any number of dimensions.

Perhaps the most valuable general notion or method which has been introduced into philosophy in the last fifty years is that of logical constructions. Two of the most exciting applications of this have been Whitehead's attempt to treat points and instants as logical constructions out of suitably inter-related volumes and durations respectively ; and Russell's attempt to treat both minds and physical objects as logical constructions, on characteristically different principles, out of a common matrix of sensibilia and images. Now it is no accident that both these contributions have been made by philosophers who are distinguished mathematicians. For the notion of a logical construction first arose and had its first successful applications within pure mathematics, *e.g.*, in the definition of irrational numbers as certain classes of suitably inter-related rationals. I think it is very doubtful whether anyone who had not been familiarized with the use of logical constructions in mathematics, and had not been persuaded of their validity and illuminating power within that region, would ever have thought of generalizing the method and applying it to philosophical problems or could have handled it successfully if he had tried.

(ii) Certain important regions of fact fall into a hierarchy of which the following series is typical : ostensibly inorganic matter ; living but ostensibly inanimate organisms (vegetables) ; sensitive and conative but ostensibly non-rational living things (animals other than man) ; and sensitive, conative, rational living beings (men). Any individual at any level in such a hierarchy has all the properties which are characteristic of the lower levels, and has also something ostensibly new and different. But the latter property is not just added to the former. It requires them in order to function, and they in turn are modified by its presence. Moreover, there are ambiguous cases at the margin of each level in the hierarchy ; *e.g.*, filter-passing viruses, organisms which one hesitates whether to class as plants or as animals, and non-human animals which seem to show traces of rational behaviour.

Now the speculative philosopher naturally wants to unify and synthesize such a hierarchy, and he is often tempted to do it in one or other of two opposite ways. These might be called respectively *Reduction* and *Sublimation*. The reductive type of unification tries to show that the features which are characteristic of the higher levels are analyzable without remainder into those which belong to the lower levels. Just the same laws hold throughout, but we have different and more special collocations of the same elements at the higher levels ; and the occurrence of those special collocations is itself explicable from the laws and collocations characteristic of the lowest level. The sublimative type of unification tries to show that the features which seem to be peculiar to the higher levels are really present in a latent or a specially simplified or a degenerate form at the lower levels. It may even try to show that features which seem to be typical of the lowest levels are partially misleading appearances of features which are typical of the highest levels. Materialism, in its non-emergent forms, and Leibniz's form of mentalism, are extreme cases respectively of the reductive and the sublimative types of unification.

The attraction of the reductive type is that the features

of the lower levels seem to be extremely stable and pervasive, both in space and time, and to be subject to laws which are easy to formulate and to handle mathematically. The features of the higher levels, on the other hand, seem to belong only to very complex individuals which can exist only under very special conditions. These conditions seem to be fulfilled only occasionally and for comparatively short periods, and then only in comparatively small regions of space. And they seem to be unstable and essentially evanescent. The attraction of the sublimative type of unification lies partly in the fact that all value and disvalue of every kind seems to reside in the higher levels. And it lies partly in the fact that the reductive type of synthesis seems inconsistent with the knowledge which men have acquired about themselves, about external nature, and about pure mathematics and logic, and with the very great control which they have gained over nature by deliberately applying that knowledge. The danger of each type is the same, viz., to ignore or to distort those aspects of reality which are not easily reduced or sublimed, as the case may be ; or to make the synthesizing principle so thin and so nearly tautological that it fits everything at the cost of illuminating nothing.

How are Proposed Principles of Synthesis Recommended ?—

The last question which I shall discuss is this. How does a philosopher persuade himself and try to persuade others to accept the kind of synthesis which he proposes ?

In former times the method was often, ostensibly at any rate, deductive. Certain very general premises were accepted by a philosopher as self-evident synthetic propositions. He either assumed that other persons would find them self-evident at once, or, if not, he tried to remove confusions and misunderstandings and to place his readers in a position in which they could contemplate these premises for themselves. He hoped and expected that they too would find them self-evident.

In recent times speculative philosophers have more and more tended to abandon this method. When one reads

Whitehead's *Process and Reality* or Alexander's *Space, Time, and Deity*, e.g., one is inclined to feel that the method may be summed up in the familiar sentence : " I'm not *arguing*, I'm just *telling* you." Each reader has to alternate repeatedly between taking a detailed view of the various regions of fact severally and a synoptic view of them collectively, and then to judge for himself whether the proposed scheme of synthesis unifies and illuminates the whole without omitting or distorting any important features in the parts. He may find that, in the light of the proposed scheme of synthesis, all the bits of the jig-saw puzzle fit together so satisfactorily that he cannot doubt the substantial soundness of the suggestion and cannot seriously contemplate any alternative scheme. He may find the whole thing a mass of tiresome and pretentious verbiage which merely darkens council. Or he may find it highly illuminating in places ; but feel that it ignores or distorts certain important features, which some alternative scheme of synthesis, though equally imperfect as a whole, does justice to and illuminates.

It is plain that such total reactions, like our judgments on a person's character or on the expediency of a policy of action, are greatly at the mercy of subjective conditions, such as temperament ; intellectual, social, and racial background ; and even liking or disliking for an individual, for his style of writing, and the persons or causes with which he is associated. An honest student of philosophy will try, so far as he can, to recognize and allow for sources of irrational bias. He will be especially on his guard when judging types of synthesis to which he is unsympathetic, either because they stir no chord in him or because they or their authors or their advocates are distasteful to him on personal, political, or racial grounds. But in the end he will have to recognize that certain types of philosophical theory are, as the vulgar would say, " not his cup of tea " ; and that he will be wise to confine himself to those systems which he can sample, if not without an occasional grimace, yet without a constant feeling of nausea. He should remember, however, that an occasional dose of philosophic

emetic or aperient may be wholesome, even though a regular diet of it is inadvisable.

Of systems of speculative philosophy which are, at any rate at first sight, predominantly deductive, Spinoza's *Ethics* may be cited as a classical example and McTaggart's *Nature of Existence* as a modern one. Hegel's *Logic* must also be included under this head, with the proviso that here the mode of deduction, by thesis, antithesis, and synthesis, is highly peculiar and would not generally be admitted to be valid. Leaving it out of account for that reason, I will conclude with two remarks on straightforward deductive systems.

(1) I am sure that the early and extraordinarily successful systematization of geometry by Euclid must be largely responsible for encouraging speculative philosophers to throw their systems into deductive form. It showed them by a brilliant example that very far-reaching and quite unexpected consequences can be deduced step by step from a few simple and almost trivial premises. Like everyone else, they took for granted that the axioms of Euclidean geometry are necessary synthetic propositions about the spatial properties of the actual world. They were thus encouraged to think that there might be self-evident synthetic propositions about other aspects of reality, or even about every possible existent as such, and that from these a whole system of far-reaching and quite unexpected consequences might be deduced.

Now the progress of mathematics and of philosophical reflexion upon it has convinced most competent persons that the axioms of Euclidean geometry are not necessary synthetic propositions about the spatial properties of the actual world. Either they are taken simply as hypotheses, or they are taken as true descriptions of the spatial characteristics of reality. On the former alternative the consequences that follow them are only entertained conditionally and are not asserted categorically. On the latter alternative they are empirically grounded generalizations; and the consequences which they entail, though they can be asserted categorically, have the same contingency as the premises.

Thus the procedure which the example of Euclidean geometry encouraged philosophers to adopt, and the hopes which its spectacular success aroused in them, were based on serious though very natural misunderstandings. It does not follow that a system of deductive metaphysics might not be constructed, as simple in its premises, as rigid in its reasoning, and as startling in its conclusions as Euclidean geometry, provided only that it was content to admit that the evidence for its premises is empirical and that its conclusions are contingent. All that one can say is that the omens seem very unfavourable.

(2) In conclusion, I want to point out and illustrate a certain peculiarity which I seem to find in many important deductive systems of speculative philosophy. It is this. The philosopher takes note of a certain characteristic, *e.g.*, extension or temporality, which seems to be very fundamental and pervasive. He reflects on it and its analysis and its implications. He thinks he can show that certain aspects of it or certain consequences of it conflict either with each other or with one or more general principles which he finds self-evident. He concludes, therefore, that this characteristic cannot *really* belong to anything. But he is left with the fact that it appears *prima facie* to be a fundamental and pervasive feature of reality, and that it continues to do so to himself and to others who accept his destructive criticism of it. He has now to "save the appearances." In order to do this he introduces an hypothesis, which may have no trace of self-evidence and may not be directly deducible from anything that he finds self-evident or regards as empirically established. He asks us to accept this hypothesis on the ground that it would save these appearances, that he cannot think of any alternative which would do so, and that he would be much surprised if anyone else could think of one. He then takes this hypothesis as an additional premise, and from it alone or in combination with the other premises of the system he deduces further consequences which are often very startling. These consequences may be among the most characteristic doctrines of his philosophy.

This procedure is very well illustrated in Leibniz's system, and I will take that as an example, though it would be quite easy to find others. In Leibniz's monadology there are at least two instances of it. (i) There seems *prima facie* to be a plurality of substances which interact with each other. Leibniz thought he could show that interaction between substances would be incompatible with certain properties which are involved in the very nature of a substance. His main reason for this was that he thought that it followed from the analysis of subject-predicate propositions. On the other hand, he saw no reason to doubt, and strong reasons for accepting, the existence of a plurality of substances. He had therefore to account for the fact that the numerous substances which there really are seem to interact with each other, which they really cannot do. He introduced the hypothesis of Pre-established Harmony in order to "save the appearances." It is to be accepted, not because it is self-evident or even particularly plausible, nor because it follows directly from anything else which he accepts; but simply and solely because it, and nothing else that he can think of, accounts for the delusive appearance of interaction. It then becomes an additional premise, from which further consequences follow.

(ii) We perceive many objects as extended. Leibniz thought it self-evident that, if anything were extended, it would be composed of adjoined extended parts. Since each part is extended, the same must be true of it. Therefore, if anything were extended, it would be composed of parts within parts within parts . . . without end. Leibniz rejected this consequence as self-evidently impossible, and he was therefore forced to conclude that nothing is or could be extended. So he has to account for the partially delusive appearance of extension. Now he had no objection to *infinity* as such; what he objected to was absence of *simple* parts. He therefore suggested that what appears to be an extended substance is in fact an aggregate of an infinite number of simple unextended substances, answering to the following conditions. Each of them has a different determinate form of a certain one determinable quality, which he calls "point of view."

There is an infinite number of determinate possible points of view, and they form a continuous three-dimensional manifold. The infinitely numerous simple substances, which together appear as a single finite continuous extended object, between them have all the possible points of view which fall within a certain limited region of this continuous three-dimensional manifold. That is why the object appears to us to be finite in extent, but continuous and divisible into parts within parts without end, although in fact nothing could possibly have the latter property.

Now I know of nothing in principle against the general method of argument which I have been illustrating. It might lead to results which were accepted by all competent persons, which were illuminating and far-reaching, and which could not be proved in any other way. But in fact it has not hitherto done so. Experts have not universally accepted the negative destructive part of such arguments. Those who have done so have not always been persuaded that the proposed hypothesis saves the appearances. And those who admit this much have not always been prepared to admit that no other hypothesis would save them.

THE CHARACTER OF A HISTORICAL EXPLANATION.

MR. A. M. MACIVER, MR. W. H. WALSH, PROF. M. GINSBERG.

I.—*By* A. M. MACIVER.

THE ultimate stuff of history is the countless individual doings of individual human beings through the ages, together with such natural events and facts as have conditioned those human doings—events such as the normal alternation of fair and foul weather or the cycle of the seasons, as well as earthquakes, inundations and droughts, and facts such as the fact that here is sea and there is dry land, this land is fertile and that is barren, this has coal and iron and that has none. But these natural facts and events are important to the historian only in so far as they condition human doings. The actual individual doings, on the other hand, collectively make up his subject-matter.

This is a plain fact which Idealist philosophers of history, with their slogan that “all history is contemporary history,” completely overlook. Oakeshott or Collingwood calls the history (let us say) of the Peloponnesian War a “mode of experience,” meaning by this his own experience in his twentieth-century Oxford or Cambridge college room, forgetting that what made the history was experience all right, but the experience of thousands of poor devils two dozen centuries ago. The Idealist philosophers have unthinkingly transferred to history an argument that was plausible enough when applied to physical science. It is easy to argue that atoms and electrons are mere postulates of theory ; nobody has ever met one in the flesh ; to say that they have such and such characters, or behave in such and such ways, is only to say that this is what physicists at present find it convenient to suppose. But, whatever may be the case with atoms and electrons, human beings are not mere creatures of theory. To say that we are now doing whatever we are actually doing, and for the reasons for which we are actually doing it, is not to say merely that this is what some future historian is going to find it convenient for his purposes then to suppose. And, by parity of reasoning, whatever men were doing a thousand years ago does not depend upon what historians find it convenient to suppose now.

Individual human doings collectively make up the stuff of history. What each one of us is doing here and now is part of the subject-matter of the history that will be written in the future. If what we were all doing now were different, the history of this period would be different. But history is not itself the record of all these doings. The historian selects and (what is even more important) generalizes. It is thanks to this that the proposition that "all history is contemporary history" is not patently absurd, because it is true that each historian selects and generalizes with reference to his own contemporary interests. But his generalizations are true or false in proportion as they represent or misrepresent all the individual doings and happenings. (This is the foundation-stone upon which I am going to build the whole of my argument in this paper—a stone which the builders of philosophies of history hitherto have, so far as I can understand them, almost universally rejected.) To show up by contrast the character of all actual written history, we may perhaps find it convenient to suppose an ideal written history, which would tell the whole story of everything that ever happened to every human being, which we might call "the Book of the Recording Angel." The function of the historian (we might then say) is not, indeed, to copy out extracts from the Book of the Recording Angel, but it is to make an intelligent précis of some part of it. This is not to depreciate the work of the historian. The function of the Recording Angel could, after all, in a fully mechanized Heaven, be performed by an electrical device, whereas the making of the historian's précis requires intelligence. But, though the précis may be made for some particular purpose and omit what is not relevant to that purpose, it must not misrepresent the contents of the original.

But, although all history rests upon this same foundation and all actual history generalizes, there are many different levels of historical generality. What I have called "the Book of the Recording Angel" may be regarded as the ideal limit to which history approximates as generalization tends to zero. At the lowest level of generality of all kinds of historical writing is biography—particularly that kind of biography which hardly professes to be more than a collec-

tion of anecdotes about its subject arranged in chronological order. This hardly generalizes at all : it differs from the Book of the Recording Angel itself only in that it selects, and that according to no fixed principle other than the accidental limitations of the writer's sources of information. Almost at the other extreme stands the sort of general world history which knows no epochs except the great technological revolutions which completely transformed the whole background of human life, for which hardly anything worth mentioning happened between the discovery of the smelting of metals at the close of the Neolithic Age and the perfection of the steam engine by James Watt. Slightly (but only slightly) less general than this is Marxian history, which considers events only in so far as they have affected or followed from changes in the large-scale organization of society for the production, distribution and consumption of economic goods. History at all these different levels is (that is to say, can be) equally good history. We do not get a better or a worse view of a field according as we take a bird's eye, or a man's eye, or a worm's eye view of it, though we get a different view, and yet they are all views of the same field.

Serious trouble only begins when levels are not distinguished. From this many futile disputes arise. Take for example the question, still often eagerly disputed, whether the acts of individuals determine the course of history. Obviously they do. The only proper question is : How much ? History is nothing but the resultant of all the acts of millions of individuals, but the consequences of some individual acts are still distinguishable after a considerable lapse of time, while the consequences of others blend together almost immediately. The fact that John Smith at a particular time on a particular morning hurried to the local branch of his own bank and withdrew a much larger sum than he would normally have withdrawn, because he had heard that another bank had failed or was about to fail, goes down to history only as part of the fact that there was a run on the banks in that week, which precipitated a world financial crisis ; John Smith's contribution can no longer be distinguished. On the other hand, the fact that General

Brown, overcome by a fit of pessimism, surrendered the strategically vital fortress of which he was in command, when a more resolute general could have held it until relief arrived, perhaps led immediately to the loss of the war and a whole string of consequences, all of which can be traced back to that single act. Certainly the loss of the war will have other contributory causes. General Brown's surrender would perhaps not have had this result if the relieving force had been in a position immediately to retrieve the situation, but in fact it was not, because it was inadequately armed, because there had been financial corruption in the quartermaster-general's department, the causes of which corruption ramify back into the whole political and social history of the country. Still the fact remains that General Brown need not have capitulated, and that, if he had not, the war could have been won, so that the loss of the war and all that followed from it was the direct consequence of this individual act of capitulation. (The case might equally have been that of an unknown private soldier, who might have prevented the enemy from gaining what proved to be a vital point, though in that case it would probably escape mention in the history-books, for the question is only whether a chain of consequences could be traced back to an individual act, if it were known.) It is possible that, if General Brown had not capitulated unnecessarily, some other commander on the same side would have done so, but this is not certain and anyhow he might not have surrendered such a vitally important position. As, however, we take a broader and a broader view, such possibilities may begin to accumulate into probabilities. If General Brown was constitutionally liable to fits of pessimism, he should never have been appointed to command a strategically vital fortress in time of war. A country whose administration made such unwise appointments might by good luck come successfully through one war, but would hardly survive a series of wars if it persisted in the habit. Where such appointments are favoured by persistent conditions, such as the general decay of a social class from which military officers continue to be drawn, resulting in turn from a cause such as the ruin of a country's agriculture by the appearance

of a more powerful competitor on the world market, it may be possible to predict that such a country will ultimately either be conquered in war or else succumb without fighting, though not precisely how or when.

We can see how this bears upon that favourite example in arguments about the influence of "great men" in history—the part played by Julius Caesar in the history of the Roman Empire. It is almost certain that, if Julius Caesar had died in infancy, someone else would have unified the Mediterranean world under a single autocratic monarchy. The situation was ripe for it. Both Sulla and Pompey had already very nearly achieved it. As seen by the universal historian who thinks in no time-unit less than a century and no social unit smaller than a whole civilization, the picture would be just the same even if Julius Caesar had never lived. For those who take a closer view, however, the picture would have been very different. If Caesar had not lived, Rome might have had to wait another generation or even longer for a man who combined the necessary ambition with the necessary abilities, and the resulting prolongation of senatorial anarchy might have had effects which would have been felt for centuries. For his actual contemporaries the difference would have been all-important.

History at different levels has different periods and different turning-points. For his own subjects the death of an individual autocrat may mark an epoch, but for later historians it may be some event in the middle of his reign, perhaps hardly noticed by contemporaries, which marks the end of one period, which began long before he was born, and the beginning of another, which continued long after he was dead, the death of the ruler himself being something quite insignificant. Marx, though he introduced the conception of the Industrial Revolution, attached no particular importance to the introduction of power-driven machinery. His historical researches were mainly concerned with the transition from feudalism to capitalism, with the ultimate object of applying the lesson to predict the course of the expected subsequent transition from capitalism to socialism. Most interesting to him was the

change in the distribution of social forces which came when production for profit took the place of production for use. The introduction of power-driven machinery appears in his account as a mere incident in the subsequent development—just one of the devices by which capitalists, in a competitive economy, sought to turn the labour-power of the workers more and more to profit. Marx duly considers its multifarious social repercussions, but, from his point of view, none of them is so important as the introduction, centuries earlier, of the new economic motive of profit. If, however, we try to look at the history of the last few hundred years through the eyes of historians living thousands of years hence, we can see at once that differences made by the transition from feudalism to capitalism will then have become almost imperceptible, but differences made by the introduction of power-driven machinery will be impossible to overlook. Yet this does not mean either that Marx was mistaken or that the historians of the future will be mistaken, but only that history divides into different periods at different levels of historical generality.

Now we can introduce the subject of “historical explanation.” By this we must understand, I think, only such explanation as is part of the historian’s business as such, and not include any further explanations in which use happens to be made of the historian’s results. In this sense, whatever “historical explanation” may be, it is not the scientific sort of “explanation” described in Mill’s *System of Logic*, Book III, Chapter XII—the discovery of hitherto unknown universal laws of which particular phenomena are instances, and the resolution of universal laws into mere special cases of other universal laws even more general. When we say that history “generalizes,” we do not mean that it seeks to establish universal laws. We contrast the “generality” of historical statements with the individuality of the facts on which they are based, meaning that they are related to those facts as the general proposition “I possess some philosophical books” is related to the individual facts (my possession of this copy of Plato’s *Republic*, and that copy of Kant’s *Critique*, and so forth) which make it true. In logical terminology, the historical proposition is “general,”

but "particular," not "universal." A typically historical statement is "The Normans defeated the English at Hastings in 1066." The battle itself was a vast medley of individual actions and experiences—this man shooting this arrow, that man avoiding it or being hit by it, horses stumbling, men feeling pain or fear or exultation—but the historical statement takes it as a whole and selects for mention just that aspect of it which bears upon the historian's purpose—in this case, the fact that as a result the Duke of Normandy was able to make himself King of England. It is not the business of the historian to "generalize" in any other sense than this.

To say this is not to give orders to historians. It is only to put a limitation on the use of the word "history" in this discussion, which is, I think, supported by ordinary usage. I think that most people would agree, for example, that in Toynbee's *Study of History*, while many of the "annexes" and incidental digressions are real "history," the body of the work is not—it is a new sort of "science." Its object is "explanation" in Mill's sense—to discover the hitherto unknown laws governing the establishment and disintegration of civilisations. This is something which only a trained historian can attempt, since it demands an immense equipment of historical knowledge, but, when he attempts it, he is going beyond history. It is possible—we may grant this hypothetically, without committing ourselves—that historical research is wasted if its results are not afterwards applied in this way ; but, even so, the application is not historical research itself, any more than the collection of social statistics is by itself the formulation of a social policy, even if it is true that statistics are wasted unless made the basis of a policy. It would be "historical explanation" to account for the rise of the Sumerian, or the fall of the Minoan, civilisation, but it is "scientific explanation" to account for the rise and fall of civilisations as such. Even if "historical explanation" is (as it may be) explanation by reference to universal laws, it differs from "scientific explanation" in taking the laws as known and concentrating upon the analysis of the particular event, asking which of the known laws actually account for it, and how, and in

connection with what other particular events. This is what a historian ordinarily does whenever he professes to "explain" an event, or the origin of an institution, or any of the other things which it is thought to be his business to explain, and I take it that the question which we are intended to answer here is, what sort of an explanation this is (or can be).

Half of the correct answer to this question is, I submit, that there is a different historical explanation appropriate to every different level of historical generality. Even in the Book of the Recording Angel there will be explanation as well as simple narrative. It will say, not merely "Napoleon was annoyed," but "Napoleon was annoyed because his breakfast coffee had been weaker than he liked it." This "because" raises familiar philosophical problems. It suggests some such major premiss as "All human beings are annoyed whenever they do not get exactly what they like," but we know that this is not in fact universally true. It is in a more recondite manner than this that the lukewarmness of the coffee "explains" Napoleon's irritation. But I will assume that the nature of this sort of "explanation" is a question for another symposium. It is not these problems, concerning the kind of "explanation" appropriate to individual human actions, which are troubling us when we are puzzled about specifically "historical explanation." We are thinking of explanation at higher levels of historical generality. But the second half of my answer to this, the question which concerns us here and now, is that correct explanation at these higher levels can be nothing but the reflection of correct explanation at the individual level. Individual acts have individual causes. This I take as acknowledged, whatever the philosophical difficulties concerning the precise kind of causation involved. Just as the historical *statement* summarises a large number of individual acts, representing a character which runs through them all—perhaps the way in which they all contributed to a particular result—neglecting all their multitudinous features which were irrelevant to this, so in the historical *explanation* some of the individual causes of the individual acts disappear as unimportant, but others add up to something which can be

stated generally. Its validity consists in representing fairly the balance of the underlying causes. The difference between a correct explanation and an incorrect one, at any particular level of historical generality, corresponds to the difference, in optics, between an undistorted and a distorted picture, at any particular degree of reduction of scale, at which some details inevitably disappear.

Historical explanation becomes confused whenever there is confusion of levels. Perhaps for brevity's sake we must sometimes say such things as that Mr. Jones votes Conservative because he is a business man ; but it is always dangerous. Mr. Jones votes Conservative for his own personal reasons, which can be indicated (though not without already generalizing) by saying that he was brought up as a Conservative, that all his friends are Conservatives, that his business experience has drawn his attention to the considerations in favour of the Conservative policy and against the Socialist, that nothing has ever happened to induce him to pay equal attention to the considerations on the other side, and that he is more biassed than he himself realises in favour of a political policy which would tend to his personal advantage. Other business men like Mr. Jones also vote Conservative, each for his own reasons, but all for similar reasons, because their situations are similar. This is the basis for the legitimate historical generalization that "business men vote Conservative because the Conservative Party represents their class interests." But it is confusion of levels to use this generalization to explain individual behaviour. Marxist historians are frequently guilty of this confusion, which results in absurd notions of "economic determinism," implying that it is impossible for an individual to have any political opinions which are not those of his economic class. Idealist historians may commit the same confusion, implying that no individual can make his own judgments but that they are forced upon him by the Spirit of the Age. For events at the individual level explanations must be found at the same level. But the generalization that the Conservative Party is the party of the business men can legitimately be used (say) to explain the decline of the Conservative Party as a natural consequence

of the decreasing importance of private, as compared with public, enterprise in the national economy. This is (that is to say, may be, provided that the facts support it) a valid historical explanation at what we may call the "Marxist" level of historical generality.

In historical explanation at all levels above the purely individual, whatever is unimportant is disregarded. This sounds subjective, but in fact there is no subjectivity in it, apart from the subjectivity of the motive dictating choice of a particular level. The test of importance, at any particular level, is a purely quantitative one. Factors are important in proportion as their influence is felt all over the field under examination. This is forgotten by those who object against the Marxian conception of history that it neglects all the spiritual achievements of mankind. So it does ; but only because they are actually negligible at the level of generality with which it is concerned. In the case of every individual there are respects in which he resembles all or very many of his contemporaries and differs noticeably from any man of another period, and other respects in which he differs from all his contemporaries and may perhaps most nearly resemble some individuals of other periods. What are called the great spiritual achievements of mankind represent that individual distinction which sets the great man apart as much from his contemporaries as from any predecessors or successors. But this individual distinction has comparatively little influence on the course of history. What influence it does have may be called "vertical" rather than "horizontal" : that is to say, the great mind influences only comparatively few individuals in any particular generation, but continues to exert that influence (generally through the survival of writings, though it might be by an oral tradition) through many centuries. The influence of Plato or Aristotle in philosophy would be an obvious example. This sort of influence is imperceptible at the level of generality of Marxian history. This is concerned with the general state of a whole society at a particular date, and its relation to the general state of the same society at an earlier or later date, and cannot be expected to attend to the achievements of individuals,

however "great," except in so far as they were immediately responsible for large-scale social changes which would not have occurred without them. And this happens even less often than it appears to do, for a movement may bear the name of a great individual, yet the part which it played in history may have comparatively little to do with him. Thus it would be absurd to deny that there is a Christian tradition which has done more to form our present ways of thinking and feeling than any other intellectual influence from the past, and equally absurd to deny that a great deal in Christianity derives from Christ, but it may well be doubted whether it is this part of Christianity which has ever had most general influence. What has determined the course of history, viewed on any large scale, at any particular period, has not been Christianity as such, but, if anything, the Christian Church as it was at that period, focussing a mass of beliefs of very varied origins, some of them old, some comparatively new (though perhaps expressed in old terms), traditions, moral intuitions, prejudices, considerations of sectional interest, and personal ambitions, in which the immediate contemporary interests of the clergy and the faithful bulk much larger than the general Christian heritage. Marxist historians can fairly be criticised for claiming (as they often seem to do) that history cannot legitimately be studied except at their own chosen level, but not for insisting that, at that level, it is only "materialist" explanations which really explain. This is a simple consequence of the fact that men are more often bad than good, and more often stupid than intelligent, so that the acts of the exceptional individuals disappear from view as soon as the human scene is contemplated from any distance.

But there is also a level at which ideas have a history of their own, which is the level at which Idealist historians prefer to work. This is quite as legitimate as working at any other level, so long as it is remembered that "absolute mind" is only a logical construction—that this sort of history is only another set of historical generalizations from the same mass of individual acts and thoughts of which we suppose the whole story written in the Book of the Recording

Angel. New conceptions and methods of approach to intellectual questions are introduced by individuals, become fashionable and are very widely applied, until finally they are found for one reason or another unsatisfactory and gradually abandoned. Changes in material conditions can exert an influence here, because they may raise new problems which the old conceptions and methods cannot solve, thus hastening their abandonment; but the importance of this is probably much exaggerated by Marxists, and the Idealist historians may often be justified in disregarding it. In any case it is very naive to think that methods and conceptions will ever be abandoned merely because problems have arisen which they cannot solve. The fact that the old methods cannot solve the new problems will not be recognised. It will be alleged that they have in fact been solved already, or else that, though not solved yet, they will be solved soon, still by the old methods, or perhaps that they are completely insoluble. What forces the abandonment of old methods and conceptions is always the invention of new methods and conceptions which prove their superiority in competition, and this requires a certain lapse of time and may have to wait for the appearance of some individual of genius. In such a case it is not misrepresentation of the facts, provided that it is recognized as being representation at a very high level of generality, to tell the whole story in terms of problems, criticisms and suggested solutions—a doctrine failing to stand up to criticism, thus producing a problem, to which various tentative solutions are offered until finally the solution is found which is associated with a great name.

But it must be owned that Idealist philosophers of history show a disposition to suppose that the history of ideas must itself be “ideal,” in the sense of describing what they think ought to have happened rather than what actually did. When the individual of genius appears, he may not be immediately recognized, and will certainly not be recognized universally. Every educated person now knows something about the greatest thinkers of the past, but the minor writers are read only by historical specialists and the great mass of the public, which wrote nothing, tends to

become quite forgotten, and in consequence we are apt to remember those who were immediately influenced by a great mind and forget that there were many at the time who never tried to think for themselves hard enough to become aware that there was anything unsatisfactory about the conceptions and methods to which they had been brought up—to whom the views of the great man were nothing but unintelligible newfangled nonsense. I may be unfair to Collingwood, but it has sometimes seemed to me that, in his account of the “presuppositions” of different historical periods, he considered only those outstanding philosophers whose achievement was so permanent that their works are still compulsory reading for Greats at Oxford. But this is to confuse what the men of a period actually presupposed with what they *would* have presupposed if they had realized that Descartes (or whoever it might be) had solved their problems, when in fact most of them did not. And this seems to have tempted Idealist philosophers of history into an account of “historical explanation” which is inadequate even to the history of ideas, to which the Idealist conception of history properly applies. They think that they have “explained” the acceptance of a doctrine when they have shown that it solved a certain problem. But to show that the doctrine actually solved the problem is logic, not history. The historical question is, how it came to be accepted, which is not accounted for by the mere fact that it solved the problem, for a problem may be solved and the solution never be generally acknowledged, while conversely doctrines may be generally accepted which conspicuously fail to solve very urgent problems.

Practising historians may be expected to dislike an account of “historical explanation” which reminds them that history rests on facts in the shape of actual human doings. This is the skeleton in their cupboard, and they prefer Idealist or Marxist theories of history, which enable them to keep the door shut on it. We may expect criticism on lines familiar to philosophers, being that used against all representative theories of perception and correspondence theories of truth. It will be said that our test of historical truth is a test which can never be made. We can never

check a historical generalization against the individual facts on which it is based, because we are never presented with both together. For immediate contemporary history or at least that small section of it which we are ourselves actually living through—we have the individual facts, but it is notorious that we always find them so complex and confusing that we cannot at the time summarize them in any general historical statement. Historical generalization becomes possible as the events recede into the past, but then the individual facts are no longer there for comparison with the generalization. From some arguments in this vein it is difficult to see what is meant to follow, if not that a historian is a purely imaginative writer like a novelist, though this is not, I think, a conclusion which any practising historian would welcome. But all that is in fact shown is that the historian's conclusions rest wholly on circumstantial evidence and are peculiarly fallible. What is asked of him is nothing resembling the absurdity demanded by crude representative theories of perception—that it should be decided by mere inspection of “representative ideas” whether or not they are good copies of “things in themselves” which nobody has ever had knowledge of. The historian cannot pretend that he has no knowledge of any individual human actions, since he is acquainted at least with his own actions and those of his friends, and we only ask him never to forget that the justification for all his statements (if they have any) is nothing but the similar actions of similar human beings, even if they lived a long time ago and in very different circumstances. Owing to lack of imagination, or simple ignorance, he may make what would have seemed to those about whom he is talking absurd mistakes concerning what they did or the reasons why they did it, and the mistakes may be inevitable in the sense that he has no evidence which should have enabled him to avoid them, but in the case of each particular mistake he might always have had such evidence that he would not have made it. He cannot claim that any of his statements is wholly true to the facts, but he also cannot pretend that there is any impassable barrier making it impossible for him to have known more of the facts than he actually

does. It is true that he cannot check his statements by the facts, but only by the evidence, which is a different thing. The facts are individual, but historical evidence is often already at a high degree of generality—for example, memoirs and dispatches, not to speak of the writings of previous historians (what are called “authorities”). Practising historians, therefore, naturally prefer what we may call “coherence theories,” according to which historical truth consists in agreement with the evidence, which they have before them, rather than with the facts, which they have not. This makes things much easier for them, just as it would be easier for the members of a jury if they could only feel that it was their duty merely to give a true verdict according to the evidence. But in fact every jurymen feels that he would be giving an unjust verdict if he condemned a man who was actually innocent, whatever the evidence. If the verdict is according to the evidence, that makes its injustice excusable, but does not make the defendant justly condemned, if he has not in fact committed the crime. Similarly the historical statement which is the most probable on the evidence available may be the best that can reasonably be expected of the historian, but, if it misrepresents the facts, it is not true. As for Idealist talk of “so-called historical facts” being nothing but what historians have said, that is only a device to increase the self-satisfaction of historians by enabling them to forget that, however near they may come to the real facts, they might always have come nearer.

These are topical questions now, not only in philosophy (owing to the publication of the posthumous works of Collingwood) but also in politics. The doctrine that “all history is contemporary history” might seem nothing more than a stimulating paradox when enunciated by Croce or Collingwood, but we may well feel doubtful about it when we see how it is officially adopted and acted upon in the Soviet Union. In 1917, according to all contemporary accounts, from whatever source they emanated, without distinction of politics, Lenin’s principal lieutenant in the Russian Revolution was Trotsky ; but, according to the history of the Revolution as now taught in Russia and to

Communists throughout the world, the second part was played by Stalin, of whom hardly any mention will be found in documents of the time. This is perfectly in accordance with the principles of the Idealist philosophers of history, according to whom what happened in the past is nothing but whatever it suits our purposes now to suppose to have happened then. Being themselves quiet Bourgeois Liberals, they thought only of quiet Bourgeois Liberal purposes and their conception of history remained mild and inoffensive. But now it has suddenly grown teeth, when it is found to have the consequence that the Battle of Hastings might come to have been fought, not in 1066, but in 1067, or perhaps even in this present year 1947, and to have been won, not by the Normans, but by the English, or perhaps even by the Russians, if that happened to be demanded by the "Party line" in the twentieth, or the twenty-first, or the twenty-second century. I do not mean to imply that a doctrine ought to be rejected merely because it has received its final polish in Russia ; but Russian ruthlessness in drawing logical conclusions does seem to me to have put it beyond doubt that this conception of history is radically false, although, as half-heartedly presented by Croce and Collingwood, it could still seem plausible.

At the root of this false conception lie, I think, two closely connected false assumptions. We are concerned with them here only as affecting history, but their influence is actually much more extensive. One is the assumption that, because we can never hope to free our opinions from all trace of error, therefore there is no truth, or the word "truth" must be re-defined and re-applied to lend dignity to favoured errors. People like to think of the truth as something which they can hope some day actually to attain—not as something which will always be beyond them, even if they can always come nearer and nearer to it—and they are ready to re-define the term "truth" to gratify this inclination. Historians do not like to think that their ideal is undistorted representation of actual past human doings, since this is something which one can do better than another, but none can do perfectly. They

prefer to make their criterion of "historical truth" agreement with the available evidence and the needs of their own time, success in this being in principle attainable. Yet it is surely obvious that a judgment may be the best that could possibly be made in certain particular circumstances, and yet false. We find it quite natural to say that the judgments of a particular historian are sound within certain limits, but in certain respects distorted by his nineteenth-century prejudices. In these respects we consider his judgments mistaken, without thinking that, living in the nineteenth century, he could have been expected to judge differently. Most of us are ready to allow that our own judgments are probably similarly distorted by twentieth-century prejudices, although we cannot say in what respects, since otherwise we should already have corrected them. When we say that they are distorted, we mean that they misrepresent the facts. If we did not recognize the ideal of correspondence with the facts, we could only say that the judgment would have been false if it had been made in the present century, but it was not, and in its own century it was true. Collingwood does say things like this when he remembers his own philosophical position, but sooner or later his natural good sense asserts itself and then we find him stating roundly that we can now see that on some points past historians were mistaken, though owing to causes which they could not help, such as lack of evidence.

The other false assumption is that, because no judgment can ever be wholly free from bias, therefore a less biassed judgment is no better than a more biassed one. In fact it is possible, even if difficult, to reduce one's own bias, though certainly not to eliminate it completely, and, other things being equal, the judgment is the more likely to be true, the less the amount of bias. (We may reflect that the world has come to a pretty pass when anything so obvious need be said, but every philosopher knows that this does not now go without saying.) But modern philosophies of history encourage historians to glory in their own bias and exaggerate it, and to approve or condemn other historians purely according as they do or do not share the same bias. There is Communist history, Fascist history and Liberal history,

and it is approved or condemned as Communist or Fascist or Liberal according to the allegiance of the critic. Thought on this subject is almost always confused by considerations of the desirability of passionate convictions for resolute action, in which it is generally forgotten that, the more fervent the heart, the greater the need of a cool head. Marx himself (though not some modern Marxists) was well aware of this ; just because he was devoted heart and soul to the Socialist cause, he insisted that an investigation of the means by which Socialism could be attained must be true to the facts and undistorted by wishful-thinking ; hence his lifelong war against " Utopian Socialists." A historian may be none the worse for studying the past with an eye to applications in the present, but we must distinguish two very different kinds of application. There is narration of past events in such a way as to encourage present supporters and discourage opponents, distorting wherever necessary for this purpose ; this is " history " according to the prescription of the Idealist philosophers, whose criterion is contemporary needs, and in fact it is not history at all, but propaganda. (Idealist philosophers will indignantly deny that this is what they mean, and I know quite well that it is not what they intend, but in that case they ought to be more careful about what they say.) The other kind of application is the discovery that something happened in the past which may serve as a guide to action in the present—indicating, for example, the likely consequences of a particular course of action. This sort of application I have already described as going beyond the business of the historian as such, since it implies the detection of universal laws in the historical process, but it does make use of the historian's results. What is important for our purposes here is that it is a sort of application which would be impossible with purely Idealist " history." The contemporary application depends upon the historical representation being true to the facts, in the sense of what really happened in the past ; otherwise it would be merely misleading. In history we may say that pure Idealism meets its Waterloo, because in history we cannot do without " things in themselves," and the problem has to be faced, how they are " represented."

THE questions Mr. MacIver seems to have put to himself are the questions how we are to explain the curious and disturbing fact that historians offer a plurality of not obviously consistent accounts of the same set of events, and of what test or tests we are to bring to bear to estimate the reliability of any one such account. If I may say so, they appear to me to concern the problem of truth in history rather than the problem of historical explanation. But I do not think that sharp dividing lines can be drawn between these subjects, and I should agree that much of what Mr. MacIver says is relevant to the problem of historical explanation in the sense in which I propose to understand it myself.

Mr. MacIver's thesis is roughly this. First, that history is an attempt to describe and explain events which happened in the past, and that the presupposition of the whole study is that the historian should at any rate try to reconstruct the past as it actually was, not merely make it up to suit his own whims or convenience. Secondly, that in this process of descriptive explanation, as it might be called, the historian makes use of general propositions, where these are propositions summing up in a single statement a plurality of particular propositions (*e.g.*, "The years from 1832 onwards were years of reform in Great Britain"). Thirdly, that the test of the truth of an historical generalisation of this sort is that it should be capable of being broken down, or translated, into less general statements, the most elementary of which would be unvarnished descriptions of individual human acts in the past (thus I suppose one would set about breaking down the example given by translating it into "The Reform Bill was passed in 1832," "The Municipal Corporations Act was passed in 1834," etc., and break these down in turn). Fourthly, Mr. MacIver suggests that the course of individual events which constitutes the subject-matter of the historian can be treated at different levels of generality according to the point of view and interests of the writer, and that no clash need ensue between accounts

written at different levels provided that the difference is kept firmly in mind. But he adds that all such accounts must submit to the same test of conformity with the facts, and is concerned to point out, by way of appendix to his paper, that the fact that we can show that this or that historical narrative is distorted by subjective prejudices is not in itself a sufficient reason for thinking that there is nothing to choose in point of truth between one history and another, or for despairing of the possibility of attaining historical truth as such.

With much of this I find myself in agreement or at least in sympathy. Mr. MacIver's idea of the different levels at which history can be written seems to me an illuminating one, and his application of it to the question of the influence of great men on the course of events offers a convincing way of escape from an ancient and, one feels, rather unprofitable controversy. His account of historical generalization, again, calls attention to an important feature of historical thinking : the fact that the historian has constant recourse to the operation Mill and Whewell called "colligation." This comes out not merely in the formation of general propositions of the kind of which Mr. MacIver has spoken, but also, as I have suggested elsewhere, in the historian's use of such general ideas as "The Reformation" and "The Industrial Revolution." Nor do I think Mr. MacIver is wrong in what he would himself say was the basic contention of his paper : that history is (or should be) essentially a narrative of what happened in the past. The idea that all history is contemporary history is one which I find as difficult to swallow as he does himself. The true historian surely makes the assumption that his task is to reconstruct a process which actually happened, however hard he may find it to carry that task out. As Professor Field has put it, the proposition "there were past events" is a fundamental proposition in all historical thinking, a synthetic *a priori* truth which the historian never calls into question.

But though I am thus in general agreement with Mr. MacIver, I cannot help feeling that he underestimates the difficulties of gaining reliable knowledge in history, and in so doing does less than justice to the opinions of those who

take a different view from his own, such as Collingwood and Mr. Oakeshott. Why I say this will come out if I turn to the main subject of our discussion, the nature of historical explanation.

History as I see it is an attempt to do two things : to say what happened in the past and (in some sense) to explain it. Now I may as well say straight off that I agree with Mr. MacIver that it is not the historian's business to formulate universal laws about human behaviour or to produce hypotheses about what sort of event is followed or preceded by what other sort. History generalizes (in the way Mr. MacIver has explained), but it does not universalize ; and in consequence it cannot be said to be a science in the ordinary sense. But, as I shall try to show, this does not mean that the historian makes no appeal to universal propositions ; and, indeed his doing so is, I think, a most important matter when we are considering the question whether objective history is possible.

At this stage, however, I wish to make no such assumption about historical thinking. I want to start from what, I hope, would be generally acknowledged, that history ostensibly consists of a narrative of individual events, a narrative in which the events are so set out that we are intended to see not only what happened but also why it happened. And I will begin by considering a remarkable and challenging theory of how this result is achieved.


This theory, the standard English version of which is to be found in the writings of R. G. Collingwood, makes very high claims for the historian. History, we are told by Collingwood, is a special form of knowledge, a form whose distinguishing characteristic is that it gives us knowledge of the individual. The thinking of the historian contrasts here most sharply with the thinking of the natural scientist. In natural science we are compelled first to ascertain our facts and then to explain them by bringing them under universal laws : we elucidate a given situation by pointing to the type of which it is an instance. But in history, we are informed, there is on such dual process. In ascertaining a set of historical facts we at the same time come to understand them. And to gain such understanding we have no

need to refer to any other set of historical facts or to any notion of what happens in historical situations of this or that type ; we can understand each situation as it was in itself. Hence of course the claim that history gives knowledge of the individual.

How does it come about that the historian is in this singularly fortunate position ? Collingwood answers that it depends on the fact that all history is the history of thought. History is properly concerned with the actions of human beings, and an action can always be seen from two points of view ; as he puts it himself, every action has both an "outside" and an "inside." Regarded from the outside, an action is an event or series of events occurring in the physical world ; regarded from the inside, it is the carrying into effect (or attempted carrying into effect) of a certain thought or purpose. It is the second aspect of actions which is important for history. The historian's business is to penetrate to the inside of the actions with which he is dealing and reconstruct or rather re-think the thoughts which constituted them. It is a characteristic of thoughts (which in this respect differ from emotions or feelings) that they can occur in different contexts : the same thought which I am thinking now may have been thought by you yesterday or, for that matter, by Julius Cæsar two thousand years ago. Thoughts, in a word, are universal. It is a further characteristic of thoughts (and this is obviously a crucial point for Collingwood's theory) that in re-thinking them we come, *ipso facto*, to understand why they were thought. If, by reconstructing the situation in which (for example) Julius Cæsar found himself before the crossing of the Rubicon, I succeed in re-thinking the thoughts which led to and indeed constituted that all-important event, I shall at the same time understand why Cæsar thought as he did ; for I shall have put myself precisely in his place, and made his thoughts my own. In these circumstances I have no need to try to illuminate Cæsar's action by referring to analogous situations in which similar deeds were done. The events I am investigating become intelligible in themselves.

I have chosen to summarize this theory at the beginning

of my discussion of historical explanation not merely because it offers a singularly clear and striking account of the subject we are considering, but also because I believe it to contain an important element of truth. I do not wish to maintain that Collingwood is right from start to finish, and I very much doubt whether he is right in his main contention, that history involves a unique and direct form of understanding which raises it above other kinds of knowledge. But I do think Collingwood was right over one crucial point : in saying that all history is (in one sense at least) the history of thought. And since I have no doubt that this assertion will be regarded as a wanton paradox, I will do what I can to defend it.

The misconception which has to be guarded against here is, I suggest, that of thinking that to say all history is the history of thought is to regard history as a fully rational process. Whether anyone would wish to make any such claim to-day I do not know ; but I certainly do not want to make it myself. I am perfectly aware (as no doubt Collingwood was, too) that human actions are not always, or even often, determined by rational considerations ; that the non-rational background to our actions, the material conditions in which we live and the feelings, emotions and passions which constitute our non-rational selves, have a most powerful effect on what we do. I am quite ready to admit, again, that a great many actions are not, ostensibly, deliberate or purposive actions ; that we often act on the spur of the moment or from blind passion. But I think it is possible to agree to all this and still defend the theory that all history is the history of thought. It is possible because an analysis of actions in terms of thoughts or purposes is only a *prima facie* analysis. When we say that every action is, from one point of view, an attempt to give effect to some thought or purpose we have not by any means said the last word about it. We are not claiming that all action is through and through the product of reason, but only that it has, as it were, a rational superstructure ; for all we know reason may, when more closely investigated, turn out to be nothir  UNIVERSITY OF TORONTO the slave of the passions, or, as might be more an instrument which

merely reflects the material conditions of the world in which actions are done. But all this can be admitted without destroying the essential contention that every action has what Collingwood calls an "inside": that it is an attempt to translate some thought into reality.

It may be argued that even this moderate form of the doctrine I am defending is vitiated by the fact to which attention has already been called, that many actions are not ostensibly deliberate or purposive. That there are such actions I should not of course deny. But I think that even these actions can be accounted for on the theory. If I am a man with a lively sense of my own importance and a quick temper and someone insults me, I may knock him down in a fit of passion. Such an action could scarcely be described as considered; yet would it be right to deny that in acting thus I did in some way envisage and bring about a certain result? Even action done on the spur of the moment has a formal thought-side; there is, as we say, an idea behind it, though it may not be one of which the agent is aware before he acts. In this connection I think it is both proper and profitable to refer to the results established in this field by the psycho-analysts. Surely one of the most striking things psycho-analysis has shown is that much human action which formerly appeared to be purposeless or merely impulsive in fact has an intelligible plan behind it. Despite all appearances it is deliberate and calculated action, though not action of which the agent himself can normally give the correct account. The paradox of psycho-analysis is, indeed, that while it has shown that human conduct is, from one point of view, much more rational than it once seemed (in the sense of having much more thought behind it), it has also shown that, from another point of view, it is much less rational than we had supposed, since so much of it can now be traced back to the operation of non-rational factors in the self.

That every action has a thought-side, and that all history can in consequence be regarded as the history of thought, is no more an argument in favour of rationalism in philosophy of history than is the assertion that we think in terms of universals, form propositions and draw inferences an argument in favour of rationalism in theory of knowledge

generally. In each case the facts seem to be clear enough, and should be admitted by philosophers of all schools, whatever axe they hope ultimately to grind. And in each case their admission clears away muddles and useless disputes, and perhaps enables us to gain a better understanding of the subject we are investigating.

In the case of philosophy of history, it is only if we appreciate the point for which I have been arguing that we can begin to grasp the nature of historical explanation. Everyone can see that an historical explanation is not, on the face of things, of the same sort as an explanation in natural science ; but in what precisely the difference consists is another question. If I am not mistaken, one difference is that the historian tends to work in teleological or semi-teleological terms. Confronted with an event which needs explanation, his first instinct is to try to see that event in its context. What this means in practice is connecting it with other events which embody the same idea or can be linked together as stages in the working out of the same general purpose or policy. And it is of course indispensable that we should be able to think of actions as each having a thought-side if this is to be done. It is the fact that the thoughts behind different actions can be identical or be otherwise intrinsically related which makes the whole process possible.

For an example of the sort of thing of which I am thinking here I cannot do better than refer to Mr. Russell's history of the 19th century published under the title "Freedom and Organisation." (Some may think it suspicious that I refer to a history written by a philosopher, but at any rate no-one would accuse Mr. Russell of having a weakness for philosophical rationalism). In that admirable volume Mr. Russell attempts to explain the events of the century in the light of the two leading ideas which give his book its title—the passion for freedom which dominated individuals and whole nations alike, and the gradual realization of the need for closer organization brought about by the increasing complexity of human affairs as the century ran its course. The first lay behind both the many struggles for liberty, internal and external, which were a feature of the first

seventy years of the century in particular, and (to some extent) the nationalistic movements which marked its close ; the second was reflected most noticeably in the transition from small-scale to monopoly capitalism (a process which Mr. Russell traces most clearly in the history of the United States) and had begun to have its influence on other spheres before the century was over (it fitted in with Bismarck's nationalism, for instance). Now I do not wish to suggest for a moment that Mr. Russell takes these two ideas as ultimate, or regards them as determining the history of the century like the different phases of Spirit in Hegel : he would explain the growth of the idea of organization, at least, by reference to the natural conditions obtaining at the time, and he thinks in fact that there are many independent factors shaping the course of history. What I wish to call attention to is simply the fact that he does make use of these leading ideas as a way of linking historical events together. I suggest that all historians, whatever their underlying principles of interpretation, proceed on these lines, and thus that the process is an essential part of every historical explanation.*

How important a process is it, and by what other processes, if any, does it need to be supplemented ? Let us turn back to the views of Collingwood. Collingwood held, as we say, (1) that all history was the history of thought, and (2) that once the historian had succeeded in re-thinking the thoughts of those whose history he was studying, he had at the same time explained their thinking those thoughts. I have accepted the first of these contentions : is there anything to be said for the second ?

The point which Collingwood and Croce (whom he follows here) are particularly anxious to maintain is that history has no plot over and above the detailed events which constitute it. To trace the course of history is at the same time to explain it, and once that is done there is nothing more to do. This view was originally developed as part of Croce's attempt to state a philosophy of immanence, and

* For some further details of the process described compare my paper on "The Intelligibility of History" in "Philosophy," April, 1942.

was meant to counter two alternatives which Croce regarded as different expressions of the same underlying error. The first was exemplified in philosophies of history of the Hegelian type, which professed to disclose the general plan which unified and made intelligible the detailed course of historical happenings. The second was the error of the 19th century Positivists, who held that all historical events could be explained by reference to a body of universal laws, in logical character of identical status with the laws of natural science. Against both these views Croce and Collingwood maintained that the historical judgment is unique and self-intelligible. So far as Collingwood (on whom I propose to concentrate) is prepared to defend this position he would do so (I think) by referring again to the dogma that all history is the history of thought. In history, he seems to be saying, we have a situation in which one mind is directly aware of another, or rather in which one mind is able to revive precisely the act of thought which was experienced by some other mind in the past. Collingwood does not wish to minimise the difficulties of attaining this relationship between one mind and another, but he seems to have believed it possible in principle, and to be the basis of all historical understanding.

But if this is his view (and I do not know that I have stated it correctly, because I am not sure that I have understood it), it is one with which I find it difficult to agree. In the first place, it is clearly incompatible with the sort of thing argued for in the first part of this paper. If the historical judgment is, as Collingwood implies, unique and self-intelligible, there can be no question of the historian's having to make sense of the events with which he deals by exhibiting them in their context and connecting them up by means of a number of leading ideas. The procedure I have in mind, and which I think is actually followed by practising historians, would not assuredly be open to all the objections brought against such a philosophy of history as Hegel's : it does not profess to link together all the events of history under a single concept, but, more modestly, works with the conception of historical periods and tries to find intelligibility within these. But the same charge of abstraction could be made against it as is made against Hegel : it is

based on the supposition that precisely the same thought lies behind separate events. To doubt the legitimacy of this assumption is, I suspect, to doubt the validity of abstract thinking itself. And here I come to my second point against the view I am attributing to Collingwood. So far as I can see, it is a view which overlooks the part played by general (universal) propositions in all thinking, including historical thinking. The fact that the historian makes use of generalizations of this sort, as well as of the sort noted by Mr. MacIver, is of vital importance for the subject we are discussing.

I will try to make clear, both to guard myself against being misunderstood and in the hope of throwing light on the process of historical thinking itself, to what universal propositions the historian makes appeal. They are, I think, universal propositions about human nature. I have already said that I agree with Mr. MacIver that it is not the business of the historian to *arrive at* universal truths : we do not find historians ending their works with a list of results stated in general terms. But that does not mean that there is no appeal to such generalizations in history. On the contrary : the historian has at the back of his mind a whole set of principles, based on his own experience, on what he knows of the experience of others, and perhaps on *a priori* considerations too, which sum up his conception of the way human beings of this or that type react to this or that kind of situation. And he uses this body of universal knowledge to a major extent in his historical thinking, sometimes making it explicit, but more often assuming it as the (more or less) common possession of himself and his readers.

Now there are some who at this point will exclaim that the game is up : that historical thinking on this account turns out not to be different in principle from scientific thinking, and thus that there is no separate problem of explanation in history. With this I cannot agree, not only because it overlooks entirely the process of the colligation of historical events which I have tried to describe, but also because there seem to me to be serious problems about this body of generalizations about human nature which I say is appealed to by the historian.

Before discussing this point, however, I must say something in defence of my assertion that it is generalizations about human nature which the historian takes for granted. This would be denied, or at least considered most inadequate, by many writers on the theory of history. A Marxist, for example, would argue that it is not the laws of human nature but the state of the material environment in which men live which determines the course of history, and that any satisfactory theory of historical explanation must bring this out. On this account what happens is shaped, in its general features at least, by factors which fall outside human nature altogether. That is why there is an "objective logic of history," dependent on the (to me somewhat mysterious) development of the forces of production, a logic which will enable us to say what will happen without any regard to the idiosyncracies of the human beings to whom it will happen. And I think that many who would not admit the whole Marxist case would nevertheless agree that material factors do sometimes operate in the way described, and that the laws of their operation are accordingly presupposed (or at any rate ought to be) in the work of the historian.

Marxists often speak as if the only alternative to their theory were a distressing kind of idealism, which flew in the face of the facts and maintained that men were solely responsible for making their own history. But surely this is not the only alternative. What one objects to in Marxists is not their insistence on the importance of material factors as a clue to historical understanding, but their assumption that these factors have a constant, unvarying and inevitable effect on the course of events. They are perfectly right to point out that the problem of production is basic in every form of human community, and that in the complicated sort of society in which we live ourselves the solution of that problem has effects in spheres quite other than those in which we should normally look for them. But it is one thing to say this, and quite another to believe that material factors are the only factors operative in history and that the laws of their operation are constant and uniform.

If there is any sense in saying that history is properly concerned with the actions of human beings ; if we are right

to distinguish processes in history from processes in Nature (as it seems to me that we must, to do justice to plain facts) : we cannot accept the full implications of historical materialism either in the Marxist or in any other form. At whatever level of historical generality we proceed, we cannot leave human nature entirely out of account. The laws we have to work with are not natural laws pure and simple, though they include statements of the ways in which human beings tend to react to different sorts of challenge set by their natural environment. For, after all, the production problem of which Marx speaks, and to whose importance in shaping the general course of history materialist historians have so rightly drawn attention, is one which has two elements in it : material conditions and the men who deal with them. If we ask what leads to the development of the forces of production which, so far as I can see, is the key to the whole Marxist dialectic of history, the answer must surely include some mention of human skill and ingenuity. And to suppose that such skill will inevitably be forthcoming if the situation calls for it is to make a very rash assumption indeed. Anyone who doubts that should study the accounts of arrested and abortive civilizations given by Professor Toynbee in his *Study of History*.

I propose to conclude, therefore, that it is, in a broad sense, generalizations about human nature that the historian has at the back of his mind when he proceeds with his task of describing and explaining the past. But I do not wish to be misunderstood here. I am *not* saying that the historian has no interest in the material background to human actions, but is exclusively concerned with motives and other subjective considerations. It is human *reactions* to different types of situation that he has in mind, and accordingly he must think both of the problems men set each other and of the problems set them by material conditions. In either case the peculiarities of individuals *may be* an important factor to be taken into account, but in neither case are they the only factor. In a word, the generalizations to which I say the historian makes appeal are not exclusively psychological in the accepted sense of the term.

Now if my general contention is right (and I do not

propose to argue it further), there are some who will ask, as I have indicated already, what the whole argument is about. Historical understanding as I have explained it includes a process of elucidating individual actions by reference to general laws ; and there it is precisely parallel to scientific understanding. In what, then, does its uniqueness lie ? If there is a positive answer to this question, it will be found by giving attention to the special character of the generalizations presupposed by the historian.

There are three different puzzles about these generalizations which trouble me : (1) How do we arrive at them ? (2) Are they constant from age to age, or are they themselves historically conditioned ? (3) What degree of reliability do we assign to them ? I cannot deal with any of these problems in a way which is at all adequate, not only because of the limitations of the present discussion, but also because of sheer lack of native ability. I shall have to be satisfied if I can persuade others that there are genuine difficulties in each of them.

(1) The project of a Science of Human Nature, which would give men a mastery over themselves akin to the mastery over physical nature conferred on them by natural science, is one with a long and respectable philosophical ancestry. Moreover, it could fairly be claimed that it is to some extent not merely a project but a reality. The programme laid down by the 19th century Positivists, of studying the human mind and human relations in a scientific way, has been carried out in psychology and the various branches of sociology ; and though the exponents of those sciences would be the first to admit the inadequacy of their present results, they would rightly say that a sound beginning had been made. Where then is the difficulty to which I refer ? It is to be found in the fact that psychologists and sociologists do not seem to have a monopoly of knowledge of human nature. There is another class of persons altogether who can claim to be experts in the subject, and the puzzle for us is that historians seem closer to these than to social scientists proper.

The persons I refer to are, of course, poets, dramatists and novelists. Poetry raises special problems of its own,

so we may concentrate here on the drama and the novel. I suppose no-one would deny that the qualifications of a successful novelist or dramatist include having insight into human nature, and that one test of a good novel or play is that the characters and actions in it should in some sense be true to life. What that sense is is not easy to define, as can be seen when we reflect that a novel like *Wuthering Heights* fulfils the requirement although, as one critic has said, its scene is laid in Hell, whilst most products of the contemporary realist school fail conspicuously to do so. The difference is to be found, I imagine, in the conviction which Emily Brontë manages to impart to the characters of Heathcliff and Catherine, a conviction so overwhelming that we accept them although we have never met anybody like them, nor are likely to. We just feel that there might have been such creatures, and that if they had existed they would have behaved as they did in the novel. It is the novelist's insight into the workings of human nature which brings this result about. And what is true of Emily Brontë here would seem to be true of every first-class novelist and dramatist, as we can see by reflecting on other cases : those of Shakespeare and Tolstoy, for instance. No doubt it was an awareness of the fact that it thus has insight into the universal laws of human nature which made Aristotle describe dramatic poetry as more philosophical than history ; though the remark does less than justice to history as we know it to-day.

But if we agree that the novelist and the dramatist do have the insight of which I have spoken, we must also agree that their having it presents a problem. In most cases they could not give a satisfactory account of the basis of their knowledge, nor could they exhibit the knowledge itself in systematic form. It is easy to say that their understanding of human beings must in some way be grounded in their experience of life ; but does that general phrase explain the whole matter ? Why could Shakespeare make so much of his experience when most of us can make so little ? How could Emily Brontë, shut up in a remote Yorkshire parsonage, acquire the experience necessary to make her conceive and understand such a character as Heathcliff's ? I doubt whether we can discuss these questions satisfactorily without

coming to talk in terms of genius, intuition and imagination. And while I should not say that the use of such terms solves any of the difficulties involved—on the contrary, it merely serves to call attention to them—I do suggest that they conceal problems to which writers on theory of knowledge might well give their attention.

The relevance of this matter to the present discussion is that the sort of understanding the historian has of human nature is perhaps closer to that of the novelist or dramatist than to that of the social scientist. It is true that breadth and depth of experience are necessary for historical understanding ; no doubt that is why history is such a difficult subject to teach in schools. Yet it can scarcely be said that the historian comes by the generalizations he uses by a series of explicit inductions, nor again that he could set out his knowledge in systematic form. He may know little or nothing of the social sciences, but he must have a considerable knowledge of human nature all the same. Whether his knowledge could be made explicit, and used to supplement or correct that of scientific investigators in the same field, is a question which cannot be raised here.

(2) So much for the first of my difficulties. To introduce the second I shall again refer back to the views of Collingwood. Collingwood combined (how consistently I do not ask) his theory of history as a re-enactment of past experience with a thoroughgoing historical relativism. Our knowledge of the past, he argued, is itself historically conditioned, and hence there is no sense in looking for a final or definitive history of anything : history has to be written anew by every new generation. This view is strikingly expressed in a passage from a manuscript of Collingwood's quoted by Professor Knox in his introduction to *The Idea of History* (p. xii) :

“ St. Augustine looked at Roman history from the point of view of an early Christian ; Tillemont, from that of a seventeenth-century Frenchman ; Gibbon, from that of an eighteenth-century Englishman ; Mommsen, from that of a nineteenth-century German. There is no point in asking which was the right point of view. Each was the only possible one for the man who adopted it.”

If we ask now in what the writers Collingwood mentions most differed, the answer will be, I think, in their conception of human nature. Each tended to regard the past through the eyes of his own present, interpreting men's behaviour in former ages by the standards and principles of his own time. Thus when we look back now at the work of Gibbon, we can see that the 18th century notion of how reasonable men behave is a most important feature of it, colouring, for example, Gibbon's whole attitude to religion. But though we hold that Gibbon was prejudiced in adopting that view and consider ourselves capable of a more objective appraisal of the matter, it is not wholly unreasonable to suggest that we are here deceived, and that instead of having improved on Gibbon's notion of human nature we have merely replaced it by a different one.

The first comment I want to make on this theory is that it seems to me sufficiently convincing to make me sceptical about the rather optimistic attitude to historical truth taken up by Mr. MacIver. Mr. MacIver asks us to imagine a sort of Book of the Recording Angel which contains a complete and undistorted record of what happened in the past. The ideal he holds out is that our histories should conform, according to their level of generality, with the contents of this volume. But he is so anxious to visit with well-merited censure those who say that because all history is biassed there is no such thing as historical truth that he fails to allow for the subjective element which history undoubtedly contains. If it is the case that men's notions of human nature change from age to age (and there is much *prima facie* evidence that they do), we have to admit the existence of such a factor. But that makes the problem of attaining truth in history very much more difficult than Mr. MacIver leads us to believe. His Book of the Recording Angel, so far from being easily accessible like the external world in the theories of naive Realists, shows signs of eluding our grasp altogether and becoming something not much better than a Kantian thing in itself.

But are we right to assume that the views about human nature prevalent in any age, which have on my argument so decisive an effect on the history written during the age,

are as much a matter of fashion as Collingwood supposed? There are at least serious arguments on the other side. No doubt it is true that there is a gulf dividing us even from our fairly recent ancestors; that the 20th century conception of man is not the same as that of the Elizabethans, to say nothing of the Anglo-Saxons or the Greeks of the Homeric age. But it remains true that there is something linking these different conceptions together; and indeed if that were not so, how could we understand Shakespeare, *Beowulf* or the *Iliad*? Undoubtedly there is much even in Shakespeare which remains opaque to us, and one reason for that is that our underlying conception of man is different from his. Yet we do recognize the human beings he describes as men whose ways are akin to our own, and that is why we are interested in his work. If there were no such basis of identity we should neither be able to understand the great writers of the past nor show any curiosity in their writings.

My tentative conclusion is therefore that the changes which take place in men's fundamental notions of human nature are not just matters of arbitrary substitution. A later age can have a genuine appreciation of the outlook of a previous generation, though it may not be able to divest itself wholly of its own subjective bias. But I realize that to try to establish this point with any conviction would require a very much more elaborate discussion than I can supply here. I can only say that I think the problem is one whose importance should be recognized by all writers on the theory of history or, for that matter, on the basis of literary understanding.

(3) The last difficulty I shall leave with little more than a mention. The point that worries me is this. Although as I have tried to argue the thinking of the historian proceeds in accordance with a system of universal judgments about human nature, it is a fact that the study of history itself contributes to our knowledge of human nature. Thus if I sit down to read a history of (say) medieval Germany I begin with certain notions of how human beings behave; but as I read on I find myself revising my notions, adjusting them in the light of the fresh evidence available. A parallel process appears to take place in the study of literature, where

again we at once begin from a given system of judgments and revise them in the course of our reading. In both cases our procedure is somewhat puzzling and unexpected, and illustrates the ambiguous character of the science of human nature, presupposed in history and literary criticism alike, yet dependent for its material on history and literature among other sources.

Whether there is really anything in this point I do not know : it may be that it could be disposed of by reference to the fact that in every science our conclusions are constantly subject to revision, though we always stand committed to a more or less clear-cut body of laws. But I shall leave the matter without discussion, and end with a summary of the general conclusions of my paper.

The crucial fact about history, as I see it, is its concern with the actions of human beings. Historical explanation owes its special character directly to that fact. Because all human actions have what I call a formal thought-side, it is possible for the historian to apply a semi-teleological method of explanation in some of his work : to explain an event by locating it in a general context of purposes and tracing its inner connection with other events of the period. This is an advantage which the natural scientists does not seem to enjoy. But explanation in terms of purposes has, for all that, a limited value in history. In the first place, it has to be carried out within a series of more or less arbitrarily delimited periods : it cannot be applied to history as a whole. It is as if the historian could find intelligibility only within small stretches of the scene spread before him, and that the wider he extended his view the more confused he became. And secondly, it needs to be supplemented by a further process which is more closely akin to explanation of the scientific type, in which we elucidate a given situation by referring to the universal principles exemplified in it. History is not itself a science in the ordinary sense of the term, but it presupposes the propositions of the science of human nature. But that this discipline, for all its importance, is by no means a straightforward branch of learning, I hope the latter part of my paper will have done something to show.

III.—By MORRIS GINSBERG.

THE issues raised by Mr. MacIver and Mr. Walsh are so numerous that I have had some difficulty in deciding on which of them I could most profitably concentrate my attention. Both of them seem to have the writings of Collingwood largely in mind. It may be well to point out that the movement for what may be called the uniqueness or autonomy of history goes back to the middle of the nineteenth century and was part of the reaction against positivism in the social sciences led by Windelband, Rickert and others and that many of the arguments adduced by Collingwood closely resemble theirs.

There seem to be two different questions. Firstly, whether in dealing with human affairs—or as the Germans say, culture—we are concerned with something which is so different from “nature” that it calls for radically different methods of investigation. If this is answered in the affirmative the conclusion would follow that there can be no “science” of human affairs in the sense in which we use the word when we speak of the natural sciences. The other question is, whether, if it be admitted that there can be a science of human affairs, there may not nevertheless be something unique and autonomous in the historical handling of human actions.

My fellow symposiasts have, I think, no doubt that there is or that there might be a science of human nature. This, however, was not, so far as I can see, Collingwood’s view. His scepticism has two roots: in the first place, he thought that the mental or social sciences have no data in the sense in which the natural sciences have. A mental fact, to be discerned at all, has to be grasped from within, and once this is done there is no further room for generalization. If it cannot be grasped from within it cannot be grasped at all and therefore cannot be used as a datum. Secondly, he thought, that the generalizations of the social sciences cannot transcend history. If ever they succeeded in establishing

uniformities these could not hold good beyond a period of history from which the facts upon which they were based were derived. The generalizations of the social or mental sciences are thus infected with a relativity which from the nature of the case, according to him, cannot be overcome. It would follow that the only way of dealing with human actions intelligibly is to deal with them historically, that is to grasp the thought which lies behind them. History would thus be not only autonomous but self-sufficient.

It has seemed to me that in dealing with these two points we should be confronted with most of the problems raised in the two previous papers. The arguments which have been adduced in favour of the view that all history is the history of thought would, if valid, be applicable equally to all other branches of sociological investigation and would lead to the conclusion that history is the only valid mode of interpreting human actions. Mr. Walsh's argument in this connection reduces—if I have understood him aright—to the contention that in explaining human events it is essential to proceed teleologically, even if the telos is not necessarily a clear or articulate purpose. The reasons he gives appear to me to be applicable to all sociological enquiry which cannot go very far without functional analysis. The question therefore arises how the end or purpose is ascertained and, particularly with reference to Collingwood's argument whether, when it has been ascertained, anything further remains to be done in the way of generalization or otherwise.

The view that history consists in the discovery of the thought which is the inner side of the event to be explained is only plausible, if at all, with reference to certain kinds of history such as diplomatic history, which is often concerned with specific acts of individuals. The historian may find it important to try to reconstruct what precisely was in the mind of Bismarck when he decided to issue his version of the Ems telegram. But it is almost meaningless when applied to the history of large-scale massive interactions such as is found in economic history or the history of language, or more generally in the history of institutions.

He would be a bold man who would venture to say by looking within his own mind what is the telos of the family or the wage-system or of punishment as part of the criminal law. The inner side of these institutions is not in any one mind but in thousands of minds in interaction and it certainly cannot be ascertained by direct inspection or self-observation. The view that the data with which history or social science have to deal are only ascertainable by re-thinking the thought expressed in them would seem to make any explanation of human affairs highly precarious. For it is the interactions between human minds and the consequences that follow from them that are important and these are not present to any one mind. If recourse be had to the extremely dubious notion of a group mind over and above individual minds it would still be necessary to show how this mind could ever be accessible to the individual mind. Consider, for example, the work of historians of languages in tracing long-range trends or drifts of phonetic changes. These often spread over centuries and even millenia and involve highly complicated actions and reactions. These may ultimately depend on variations occurring in individual minds but the cumulative forces which selected these or those particular variations are not discoverable by the methods of individual psychology.* So it is with all the major social institutions, the changes which they undergo may ultimately be traceable to changes in the minds of the individuals sustaining them but the institutions have, so to say, a structure of their own, which reacts on individual minds, and which must be studied as such.

In urging that social phenomena cannot be explained merely by re-thinking the ideas that lie behind them, I am not asserting that they have not a mental side to them or that they can be understood without reference to function or purpose. I want to say rather that the mental factors can in most cases not be reached directly and that in any case they are not the only ones to be considered. Thus I

* Cf. Sapier : *Language*, Chapter 8, *Language as a Social Product*.

should say we cannot understand a movement like Nazism merely by penetrating into the minds of Nazi leaders and their followers. The presence of a number of individuals with a neurotic lust for power and a larger number of individuals overcome by a feeling of powerlessness and anxiety may have provided the psychological background, but a full account of the rise of Nazism would have to take into account the economic and political conditions without which the same psychological factors might well have found another expression.

It may be well to refer in this connection to the views of Max Weber. He, too, was of the opinion that sociological and historical explanation consisted in understanding or interpreting behaviour. By this he meant the direct apprehension of the intention of the agent such as we experience, say, when we see a man striking another in anger. Such apprehension was ultimate and direct and was not in need of being made more intelligible by generalisation. Weber, however, was well aware that the imputation of intentions and still more of motives may be a very risky matter, since in the same situation different individuals may well behave differently and the motives from which they think they act may not be the motives which in fact inspire them. It is therefore necessary to supplement interpretation in the sense of "understanding" the actions by explanation in causal terms. The interpretation has, as he puts it, to be shown to be "causally adequate" by reference if not to laws at least to frequently observed successions, suggesting that such and such conditions favour such and such modes of conduct, or that such and such economic conditions are linked with such and such moral and religious attitudes. In the absence of such causal investigations the effort to understand by direct intuition might easily lead the investigator astray.

With a great deal of what Mr. Walsh has to say in his defence of the view that in a sense all history is the history of thought, I agree. It should be remembered, however, that thought is used by him in a very wide sense to cover all grades of purposive action including low levels of conation.

This seems to me to differ radically from Collingwood's view, which is based on a sharp separation of the rational from the non-rational elements of the mind. These non-rational elements were, according to Collingwood, no doubt parts of human life, but not parts of the historical process, history being always of thought and of thought alone (*Idea of History*, p. 231). I can see no reason for accepting this sharp distinction between sensation and thought, impulse and will and the view that in studying human actions we are concerned with thought alone seems to me to have no relation to the work actually done by historians and sociologists.

I conclude that the data of history and of social science are not self-explanatory. They do not consist just of thoughts which can be re-thought by the investigator. Thought does not operate in a vacuum. It arises within the field of impulse and in response to fundamental needs. It is affected by the material on which it works and is conditioned at least in direction by social forces. The assumption that mental facts can be sufficiently grasped, "from within" by a sort of self-knowledge loses all plausibility once it is realised that thought and sense, impulse and will are intertwined and that to understand them self-observation has to be checked by the observation of others. The data of sociological investigation, moreover, do not consist only of individual human acts. They also include elements of the social structure which, though the result of innumerable inter-actions between individuals, comes to have a character of its own and to mould future acts. The relations between the parts of the structure and the changes which they undergo are the proper object of both historical and sociological investigation.

Like Mr. Walsh I find Mr. MacIver's comments on the different levels of historical generality very helpful and I agree with both of them that it is not the business of the historian to establish universal laws and that in this respect his aim differs from that pursued in the natural and social sciences. Nevertheless, the contrast must not be drawn in too sharp a form. The theory of the evolution of plants and

animals or the theory of the formation of the earth describes an individual series of events. Is it history or science? Furthermore, in some historical studies the discovery of general laws, or at least regularities and the interpretation of past sequences are not sharply separated. For example, I take it that the laws of phonetic change were discovered as a result of, or perhaps as a direct part of, the historical study of the growth of languages. Finally, the interpretation of particular facts need not of course be confined to the past and ought not therefore to be taken as the distinguishing characteristic of historical explanation. Collingwood, by the way, does not subscribe without qualification to the view that the business of the historian is to give an interpretation of individual concrete events. The individual can never be grasped as a whole. All that the historian can do is to rethink the thoughts behind the acts of the individual and even this he cannot grasp in its entirety, "just as it actually happened." He can only grasp that in the thought which can be shared by others and by re-thinking it make it his own. This common element, Collingwood argues, in what I have found a difficult section of his work, is not an abstraction in the sense of a common characteristic considered apart from the individuals that share it. "It is the act of thought itself, in its survival and revival at different times and in different persons." As such it oversteps the limits of merely local and temporal existence and possesses a significance valid for all men at all times. By this he appears to mean not merely that different men can think of the same object but that the act of thinking itself "somehow stands outside time," yet can endure through a lapse of time and revive after a time when it has been in abeyance. This appears to imply a mystical unity and continuity of mental processes over-leaping individual personalities which I find it difficult even to grasp.

With regard to the alleged relativity of historical knowledge, it must be admitted that the historian is influenced in setting his questions and in selecting his data by his own judgment of what is important or significant, and in this he may easily be affected by subjective factors, due

to his own mental make-up or to the climate of opinion of his age. But this is a matter of degree and I agree with Mr. MacIver that a great deal can be done to correct, if not to eliminate, errors arising from bias thus caused and that it is a sad state of affairs when anything so obvious needs to be said. As far as Collingwood's attitude is concerned, it seems to me that what he is anxious to stress is not the relativity of historical knowledge but rather that of the mental or social sciences. He does not indeed claim finality for history, since, as he says, changes in method and the advance of knowledge in history itself and other fields may reveal new sources of evidence and set fresh questions, with the result that each age has to think out its history afresh. But lack of finality, he adds, does not justify scepticism : it only means that the historian must recognize that he is himself part of the process he is studying and that history itself must be studied historically. (*Idea of History*, p. 248). But this self-correction is, strangely enough, not open to the science of human nature, so that they can never overcome the relativity inherent in them. I cannot be sure that I have understood his argument, but I take him to imply that there can only be a science of human nature if human nature were not subject to change. "The mental scientist," he says, "believing in the universal and therefore unalterable truth of his conclusions thinks that the account that he gives of mind holds good of all future stages in mind's history ; he thinks that his science shows what mind will always be, not only what it has been in the past and is now." (p. 220). I cannot see that the mental sciences need assume anything of the kind. If in fact it turns out that human nature undergoes change, this change is something that mental science will have to study and if the laws governing such changes can be ascertained, the laws themselves need not change except in the sense that they may be subject to revision in the light of advancing knowledge. It may well turn out that in historic times there has been no change in human faculty as such but only in the manner and form of its expression. To settle such questions extensive comparative studies are needed

and in these history and comparative sociology must work hand in hand. It is interesting to note that despite his repeated insistence on the variability of human nature Collingwood gives few indications of what sort of changes he had in mind. The attempt made by Lévy-Bruhl to establish a radical difference between the mentality of primitive peoples and those of "civilised" peoples might be of importance in this connection. As far as I can judge this attempt has failed. Lévy-Bruhl's analysis reveals no fundamental differences in mental structure and function but at most only a difference in the ways in which the rational and non-rational elements of the mind are balanced under different social conditions and at different levels of development.* Such studies as have been made of national and racial characteristics do not, as it seems to me, reveal any fundamental differences in instinctive or emotional equipment, but again only differences in the way in which the inborn tendencies express themselves and balance each other under different social conditions.

Since Mills' arguments in favour of what he calls "ethology" it has been recognised by sociologists that the general laws of psychology, if they were to form the basis of sociological explanation, would have to be supplemented by a study of the characteristics of various types of human groups and of the way in which these characteristics are moulded by, as in turn they mould, institutions. The task of social psychology, which I take to resemble that which Mill assigned to "ethology" is to show how the capacities, desires and emotions of man change and develop as a result of social processes and at the same time how through innumerable interactions they shape or constitute the social processes. Such a social psychology must rest on comparative studies of existing and past societies. Only if it were fully developed would it become possible to interpret human behaviour in the sense of disclosing the mentality behind it. If social psychology and the other social sciences

* Cf. the very penetrating remarks made in this connection by J. Haesaert, *Essai de Sociologie*, pp. 226 seq.

cannot claim to transcend history, it is equally true that historical explanation will remain insecure until the sciences have reached greater maturity than they have so far attained.

I conclude that no sufficient reasons have been adduced to support the view of the uniqueness and autonomy of history. History is concerned with the ascertainment of particular facts and their interpretation. Such interpretation must make use of principles at different levels of generality. These principles are not necessarily exclusively psychological or even teleological : there may well be social laws *sui generis* defining the relations between the parts of the social structure to each other and to the whole and of the changes which the structures undergo. Thus, explanation of historical facts is ultimately similar in kind to the explanations which the social sciences seek to attain. The social sciences must use the facts provided by historians as data in precisely the same way as they use the facts provided by the observation of contemporary societies and apart from the difficulty of checking observations or obtaining fuller evidence, I cannot see that there can be any difference in principle between historical interpretation and the interpretation of the facts of contemporary societies. It has struck me as odd that those who insist that all history is contemporary history do not proceed to enquire how in fact contemporary situations are investigated. In dealing with particular situations in the contemporary world aid is necessarily sought from all the social sciences and the techniques which they have developed. This is, I think, also coming to be the case in historical work, which makes increasing use, for example, of economics and demography. Finally, if the study of human actions necessarily suffers from a certain relativity of outlook this affects history as well as the social sciences and in both cases it admits, in varying measure, of self-correction.

ARE NECESSARY TRUTHS TRUE BY CONVENTION?

MR. K. BRITTON, MR. J. O. URMSON,
AND MR. W. C. KNEALE.

I.—By KARL BRITTON.

Introduction.

CONSIDER the three propositions :

- (1) If this is water, then its composition is H_2/O .
- (2) If a person is a grandparent, then he is the parent of a parent.
- (3) If A is longer than B and B is longer than C, then A is longer than C.

I think it would be generally agreed that the second and third are necessary propositions ; that the first and third are synthetic and the second analytic. It seems to me that the first might sometimes be regarded as necessary, that is to say, as true by a definition of " water " which it would in some cases be proper to apply. Then I suggest that in each case, the acceptance of the propositions as necessary is shown in the adoption of certain procedures of inference. Such procedures are called for in a variety of particular contexts, and the following are only typical illustrations :

(a) If I were offered the result of an experiment which purported to show that water had some other chemical composition, I should be inclined to reject that result and to say that the experiment had been carried out incorrectly.

(b) If I were told that a person was aged 25 and was a grandparent, I should say that was a physical impossibility, since a person cannot become a parent before he is 15, and so must be 30 to be a grandparent.

(c) If I had measured A and B and found A longer than B, B longer than C, then anyone else's measurement showing that A was not longer than C would lead me to say : " Either one or more of our measurements was not correctly made, or the objects A, B and C have changed in length between the measurements."

In each case we have a connection between the acceptance of a proposition as necessary, and the adoption of certain procedures of inference or transition from one statement to another. I call these *procedures*, because they can be done correctly or incorrectly.

In the case of (1) it is easy to see that we adopt these procedures because of a belief which we hold. This belief is about a physical constituent of what the vulgar call "water"—*i.e.*, a constituent of rain-water, sea-water, river-water, etc., which can be identified by many physical properties, as well as by its chemical composition. And the procedures show that we (not unnaturally) attach greater confidence to the well-attested results of countless careful experiments than to the result of any one particular experiment. But it is well understood that this preference can be altered; that experiment *could* lead us to revise our belief and so to revise our definition also. I suggest that the sentence (1) is sometimes used to convey an empirical proposition and sometimes to convey something that is true by definition.

In the case of (2) and (3), we should say that no experiment could lead us to change our procedures. And there seems to be little mystery as to why this is so in the case of (2). For the evidence which would lead us to say that a person is the parent of a parent, is just the same as the evidence which would lead us to say that he is a grandparent; and what would shake one assertion would equally shake the other. So that, while (2) can be used *as a premise* in an argument about parents, etc., the same argument can always be conducted without this premise, if we follow *a rule* of transformation according to which the expression "parent of a parent" can be replaced by "grandparent." Such a rule is called a definition:

(2') For "parent of a parent" write "grandparent."

Is there anything involved in accepting (2) as necessary, beyond adopting the procedures laid down in (2')? I think it would generally be claimed that there is something more: that (2) is not to be confused with the definition (2'), since a definition is not a proposition and (2) is a proposition

—it is, in fact, a proposition derived from the assertion of an identity—"the parent of a parent is the parent of a parent"—by applying the definition (2') to one side of the equation. If so, the "more" that is involved in accepting (2) as necessary is nothing more about grandparents and parents specifically. It is a completely general truth belonging to formal logic. I shall discuss the necessary truths of formal logic in a later paragraph ; in the meantime it is to be noted that an objection or query about the connection between parent and grandparent might arise from a misunderstanding about our use of "if—then—" instead of (or as well as) from a misunderstanding about our use of "grandparent" and "parent." And for the clarification of our use of "if—then—" the form (2) is very much more useful than the form (2'), for (2) is a kind of example or picture, and allows us to show the order in which we use the words—an essential matter in the giving of a rule about sentence construction and combination, and often important in giving rules about non-formal words also. So that even if we were to decide that the "more" involved in accepting (2) as necessary, is something more about procedure, we might still insist upon the importance of the form (2). But merely for explaining the connection of "parent" and "grandparent," the form (2') is certainly as valuable as form (2) and alternative with it.

Why is it that no experiment could lead us to change the procedures associated with (3)? Is there anything involved in the acceptance of (3) as necessary, beyond adopting these procedures? The orthodox view is that (3) is not merely used to convey or express certain procedures, but does itself stand for a proposition that can be believed ; and that the procedures are governed by this belief in somewhat the same way as the procedures associated with (1) are governed by the belief that water has a certain chemical composition. The belief expressed (or supposed to be expressed) by (3) is not a purely *general* one, but is about what the vulgar call "length." But, in contrast to the belief about what the vulgar call "water," the belief about "length" cannot be upset or confirmed by

any kind of experiment. It is a necessary proposition of applied mathematics.

I should like to take it for granted, then, that the acceptance of (1), (2) or (3) as necessary is shown in the adoption of certain procedures of inference, and that in the case of (1), these procedures are based upon an empirical belief which is an alternative interpretation of the sentence (1). I want to consider the question: What *beliefs*, if any, are involved in the acceptance of (2) and (3) as necessary? These two may be taken as examples of necessary truths of pure logic and applied mathematics respectively. In addition to these I shall take

$$(4) \ 5 + 3 = 8$$

as an example of a truth of pure mathematics.

What is needed is to show in detail what *use* we make of (2), (3) and (4); how we, in fact, understand, accept, and apply them, and in what ways we can check the accuracy of the results we get by applying them. I shall argue that we *understand* necessary propositions if and only if we are able to carry out the associated procedures; that we accept them as necessarily true if and only if we recognise them as rules for procedures which we all in fact carry out or try to carry out; but that there is no occasion or opportunity to check the results of these procedures by any sort of comparison with fact. The check is rather in the comparison of our results with those which other instructed people get by following the same rule. So that if we are to speak of these necessary propositions as "true," then the test of truth is simply *general agreement*.

It will be seen that, of the three (or four) sentences I have listed, sentence (1) is the only one which I believe to be used to express a true-or-false proposition, and that only when it is understood as empirical and not as necessary. I hold that the sentence (1) is often, and the sentences (2)–(4) are always, used to convey a rule and not a proposition that can properly speaking be called true or false. For we can say that something done is correct or that it is not correct, according to a rule; and we can say that this

is "the rule to be applied here" or that it is *not*. But a rule is not verified by being carried out, nor falsified if it is broken or if it is applied in a situation to which it does not apply. Nevertheless, having made this explanation, I ask for some latitude here. For, whereas we do not properly speak of a definition as true or false, we do normally speak of an analytical statement constructed on such a definition as being a proposition and as necessarily true. In my view, a necessary proposition is an illustrative sentence which is used in formulating, teaching and applying a rule of inference. The sentence is actually used as a premise. And if the sentence illustrates *the rule that is to be applied here*, then we speak of it as meaning a necessarily true proposition. I feel disinclined to straddle such truths and such propositions with officious quotation marks, because I am content to admit that these words have more than one (indeed, many more than two) perfectly correct uses. But I hope this apology makes it clear that I face up to the fact that, on my view, necessary propositions are not propositions and are not true or false in the same sense as empirical propositions.

In arguing for a conventionalist interpretation of necessary truths, it is important for me to say that I am not prepared to equate such propositions with empirical generalisations about the way in which people actually use words and make transitions from one statement to another. I shall argue that a necessary proposition always lays down a rule and that a rule is never to be equated with an empirical proposition. It will also be important for me to meet the objection, made in particular against this interpretation of logical truth, that we could not possibly draw up a set of rules for inference unless we already knew the form they must take. On this objection, my view is that of course we must be acquainted with a procedure before we draw up rules for it; but this does not mean that the rules *describe* the procedure, in the way that the propositions of empirical psychology describe *processes* of thought or action.

The view that the following remarks are intended to support has already been refuted at philosophical discussions

in London and at St. Andrews ; while Professor Max Black, breathing out threatenings and slaughter, has pursued it as far as Illinois. Upon such a question, however, Cambridge must be considered as the highest court of appeal ; and I can but marvel at the curious freak of chance that has condemned me to be its advocate in such a place. With suitable modesty, I shall do no more than try to move forward along three separate lines suggested by three of my examples.

Necessary Truths of Applied Mathematics.

The procedure which I described in connection with the proposition (3) is one to be adopted in a case of disagreement of the results of measurement. A precisely parallel procedure would have to be employed in a case where the results of different *counts* were not in agreement. *E.g.*, I count two groups of cows and find five in the first group and three in the second ; then the two groups are mixed together and you count them and find there are now nine cows. We then ask : Did one of us count incorrectly, or has another cow entered between the two counts ?

Such a procedure would not be adopted unless there were some independent reason to suppose that we are both confronted with the same unchanged group of objects. If, in such circumstances, I measure A, B and C and find A longer than B, and B longer than C, and you then independently measure A and C, and find A longer than C, we shall say that your result *confirms* mine. But if your measurement showed that A was not longer than C, we should interpret this disagreement either as mismeasurement or as a sign that A, B and C had not, in spite of other evidence, remained unchanged between the measurements. That two results (of measuring or of counting) agree, is a matter of fact ; that in certain cases they are taken as confirming each other is a matter of procedure. Similarly, that disagreement is taken to mean either that both cannot be correct, or that the two results are proper to different groups (and so not comparable) is likewise a matter of procedure.

I think it is clear that all synthetic necessary propositions which seem to say something about physical objects in the world, are associated with some procedure or procedures of counting or measurement. These necessary truths presuppose both that the procedure has been carried out correctly and that the group has remained constant in a certain respect (in respect of length, weight or composition, etc.). But even if the results merely disagree and there is no other evidence at all, either of wrong measurement or of instability, we are never obliged to say that our necessary proposition has been falsified. For that the results disagree is itself good evidence that one or other of these alternatives is true ; we have merely to decide (if we must decide) *which*.

This is not to suggest that we do not learn anything from counting or measurement. What we learn is, that in certain physical conditions, which can be independently identified, a given kind of system remains constant in respect of the measured property. So that we can expect to *explain* a variation by reference to other physical changes. If, for instance, I find on re-measuring the length of a tram-rail, that it has diminished slightly, I suspect that its temperature at the time of the second measurement was lower than at the time of the earlier measurement. And if, on recounting my flocks at the end of the season, I find more sheep than I had at the opening, then I may offer up my thanks to the god who gives the increase. On the other hand, I know that a book cannot double its pages while it sits on the shelf, nor billiard balls multiply after their kind on the billiard-table. These are empirical facts which we have all found out. They show a regular correlation between facts of measurement and other facts. They show what it is worth while to count and measure, and what it is not.

But there is something else which we learn from counting and measurement. We learn about our own accuracy in carrying out these procedures ; that this child cannot yet count large collections, that this balance will not weigh very small masses. For in the vast majority of cases, dis-

agreement in results can be correlated clearly enough either with incorrect procedures or with instability. But suppose it cannot? Then something unaccountable has gone wrong with the procedures or some miraculous change has occurred to the group we are measuring.

Miracles are extremely rare. If it were not so, we should have no clear notion of when two counts or measurements were in agreement and when they were not. And unless we had such a notion, and unless it were regularly possible to reach such agreement, then counting and measurement would not be procedures at all. For we should be unable to agree as to when they were being carried out correctly and when not. But it is essential to what we mean by a "procedure" that we can say, "This is correct" or "This is incorrect," of what purports to be an instance of carrying out the procedure. And the test of correctness, in the case of any procedure, is precisely this *agreement in the results obtained*.

It might seem that there ought to be some other test of the correct carrying out of such a procedure as counting or measuring. One might say: "But if I count according to the rules for counting, then my result must be correct and must be confirmed by any other count made according to the same rules; and if anyone gets a different result it may be dismissed, however often it turns up." But it is not so. If two people count the same group and get different results, how do we decide which has counted correctly? It may well be the case that each of them knows how to count and appeals to a correct formula in justification of his result. We decide between them by considering their results (whether the result reached at the end, or the results of each separate stage taken one at a time) along with the results of a recount and of as many further recounts as we care to make. And what assures us that one result is correct and the others not is that there is at last general agreement upon that result and upon no other.

What assures us that we all follow the same procedure in counting or in measurement, that we all understand the rules in the same sense, is that we all get the same result

in the same circumstances. This explains why *agreement* plays such an important rôle in the application of the truths of mathematics by means of such procedures as counting, measuring, weighing. If I count a group correctly, and you count the same group correctly, it is not an empirical proposition that we both reach the same number. For the *test* of the correctness of a count (as of all procedures) is that the results shall be generally agreed.

Necessary Truths of Pure Mathematics.

Counting may be compared with adding : in each case we do in fact reach an agreed result at once where the number of items involved is small, but where the number of items is very large, there is often a preliminary disagreement. Just as we can find out how many sheep are on the hill only by counting, so we can find out what is the total of a column of figures only by adding them up. If the column is very long, accountants sometimes disagree as to the total and go over the figures again and again until they all agree. Of course, they may check each other's addition in the way one might check a child's counting—in detail, step by step. But this is only a more minute application of the test by results. The question at each step is : "What result do you get by applying the rule of addition to these two numbers ?" It is only *in this sense* that "the rule of addition determines the correct answer," *i.e.*, not the rule but the way in which it is applied determines the correct total. And a necessary condition of a correct total is that it be agreed upon by all instructed persons who have made the addition.

Suppose I reach one total, and everybody else agrees upon a different total. Of course, I cannot be satisfied with this situation and must try and try again to see where I go wrong. If, after the utmost care, I still cannot see how they reach their total, what can I say? I can say (what they will say) that I cannot add, that the procedure is one which I have not managed to learn even though I can repeat a rule of addition without difficulty. In other words, I do not understand the rule of addition in the

same way as other people. Shall I then (as courtesy might suggest) accept their total as the correct one? This will be no more than a pretence: for their statement *that these figures total so much* is precisely the one which I do not really understand; it is like a sentence from a foreign tongue, *some* of whose uses I know. I may manage to make good use of it, myself, but it is a precarious matter.

There are, of course, important differences between truths reached by counting or measurement, and truths reached by pure calculation. Disagreement as to the latter can never be explained by reference to the instability of any physical system; it must always be explained by faculty calculation, and can be set right only by recalculation. Is it the case that $29^2 = 851$? I can, of course, always vary my method of calculating, but that the two methods used are equivalent is itself to be determined by calculation. There is no stage at which a wholly external test is applicable. Contrast the verification of the empirical statement that the 29th book on my second shelf is "Emma." Here we begin with calculation, but we go on to observation. If what the statement says is true, then there will be the red cover of "Emma." (As Quine might say: "'Emma' has non-vacuous occurrence in the statement": and "The 29th book on my second shelf is 'Persuasion'" is *not* a vacuous variant of the original statement.) It is precisely the absence of such an external test which leads us to speak of certain propositions as necessary, and which led Descartes to accept a subjective test of their truth: "The things which we conceive very clearly and distinctly are true."

The test of the correctness of " $29^2 = 851$ " is the agreement of all instructed people; and I am not using the phrase "all instructed people" to beg the question. For one can easily *imagine* a case in which people who had all been instructed in a method of calculation could not all agree upon a single result in a given case, but were evenly divided in their adherence to two incompatible results. In that case, certainly, the test of agreement would give a negative result. Neither group would be likely to

claim that their own result had been conclusively established. If we add, "But surely they could come together and by discussion discover some source of their disagreement, and so finally come to an agreement," then we are only insisting that the case is an imaginary one, and that, in fact, we do reach agreement by the methods of calculating described in books of mathematics.

We test the correctness of " $29^2 = 851$ " by applying the rule for squaring numbers, to 29 (which we recognize to be a number), or by applying a rule for extracting roots, to the number 851. A rule cannot itself give us the results of its own applications. This is of some interest since a rule is normally taught by giving examples of how the rule is applied. Thus I might explain how to multiply a number by 2, by saying, "You add it to itself, thus $2 + 2$, $3 + 3$, $21 + 21$, and so on." But a rule cannot include *all* the results of its own application; and the whole idea of the examples is to show us how to apply the rule to cases not given in the formula. So that a rule is not the same thing as a *summary* of all the results that can be obtained by applying it.

Of course, in addition to the examples which show the rule, a textbook commonly contains a series of theorems or a table of equivalences, which enables the beginner to apply an independent test to his own calculations. Thus, on being introduced to the calculus of *Principia Mathematica*, a reader might ask himself,

Does $(p \cdot q) \supset \neg r$ imply $(p \cdot r) \supset \neg q$?

The answer to this question, and to almost any such question that is likely to occur to a beginner, is to be found in the theorems. But what is the *theorem*, and what is a *table* of multiplication or of logarithms? It simply embodies results which have in fact been reached by the application of the rules explained in the text—reached by the author or copied by the author from somebody else. And they are used as a test, on the assumption that they have been verified by all instructed and competent students. If this is indeed the case, then a reader who cannot manage to

reach the given result by applying the rules in the same case may infer that he has not yet learned the meaning of the symbols, *i.e.*, the rules for their correct use. And it would be precarious for him to "accept the answer given on the authority of the book," because he cannot do anything useful with it until he does understand these rules. I need not further describe a situation so familiar to anyone who has tried (without any particular talent) to follow the orderly maze of symbols in a modern logic.

I have tried to explain what I understand by "a correct application of a rule." But the necessary truths of mathematics and of logic presuppose not merely the correct application of a rule, but of *the rule* that is to be applied here. What does it mean to say that a given rule is *the rule that is to be applied here*?

Suppose a beginner in arithmetic proposes to himself to divide 57,469 by 4, and proceeds as follows :

$$\begin{array}{r} 4)57,469 \\ 11,112 \\ \hline + 13\ 21 \end{array}$$

and so reaches the unorthodox result, $11,112 + 7$. He has correctly followed a simple rule—a rule which anyone could learn in two minutes. What distinguished his procedure from the orthodox one which gives the result $14,367 + 1$?

This is a most important question. I think part, at any rate, of the answer, can be given in two empirical propositions :

(I) Our *habitual* use of language does in fact conform to the "orthodox" rule.

(II) This use of language is not merely a habit : we deliberately try to conform to the orthodox rule. This is seen, for example, when we appeal to this rule to justify what we have done ; or accept an alteration of what we have done when we notice that we have accidentally broken the rule.

We could, along the same lines, distinguish between two ways of spelling a word, *e.g.*, "ghost" and "gost." And

in the case of this spelling, I think (I) and (II) together tell us all we wish to assert when we say that the rule for spelling the word with the "h" is the rule that actually applies.* We should be unable to "justify" the orthodox method by appeal to any wider rule, *e.g.*, about the spelling of words which have a hard "g" sound in them. But in the case of the two rules for division, it is very obvious that we should go on to justify the orthodox rule by appeal to much wider considerations. We should say :

(III) The rule that is to be applied here is itself an instance of a more general rule, *e.g.*, a rule connecting division with multiplication, and a rule connecting multiplication with addition, and so on. Part of what we mean by saying that it is to be applied here is that it conforms to these other rules.

It may be asserted as an objection to (III) that the unorthodox rule is in fact connected with other rules (*e.g.*, the orthodox rules of multiplication and of division, or other unorthodox rules) by *some* wider rules—but not the orthodox ones. For we must not assume that the new rule about division of numbers *must* be regarded as incompatible with the orthodox rules of multiplication and addition. Whether it is or is not so regarded is something that is determined by those who co-operate in carrying out the new rules. We have then to say what we mean by saying that the orthodox wider rules are the rules to be applied, and not the unorthodox wider ones. We may in this way push the enquiry back, but we shall in the end have to be satisfied by saying that the wider rules are to be

* It is quite possible that the spelling "gost" is correct for certain purposes, *e.g.*, as a surname. In a similar way, a rule that does not apply in addition may apply in subtraction, and a whole set of rules that has no application in mathematics has application elsewhere. In order to certify a principle as a necessary truth one must know *what particular use* is made of it. Clearly, we cannot distinguish one set of rules as valid and another not merely by examining the structure of the set of rules. And rules for quite different purposes may actually have the same structure.

applied because they satisfy the requirements (I) and (II), *i.e.*, we do conform to them and conform deliberately.

It will be seen that (I) and (II) are empirical propositions, and that (III) is a *ruling*, *i.e.*, a statement that such and such follows from, or is the result obtained by applying, a given rule or rules. But (III) does no more than relate one rule to others, and the question why *these others* are binding has in the end to be answered by empirical propositions of the same type as (I) and (II). It follows that it is a possibility that other rules might have been applied, hence, that propositions other than the necessary truths with which we are familiar might have been necessary in their stead. For the truth of a necessary proposition rests upon two stages of agreement: We agree in trying to apply a certain rule, and we agree in the results we get in applying it. In other words, we all carry out the same procedures and accept the rule or necessary proposition as a formula to indicate and to teach that procedure.

This does not mean that I regard a necessary proposition as itself a disguised empirical proposition about what we actually do. The necessary proposition itself *gives a rule*; the empirical statement that the proposition is a necessary truth presupposes the understanding of the rule. For in order to say that habitual usage conforms to the rule, I must be able myself to say: "Here the rule is being carried out, there it is not." And I must similarly be in a position to give rulings under the unorthodox rule, in order to be able to say that habitual usage does not conform to it. Otherwise my survey of habitual usage could be no more than a statistical account of what people actually do on different occasions.

What distinguishes the proposition " $3 + 5 = 8$ " from the proposition " $3 + 5 = 9$ " is that in the first case we do all actually reach agreement in our application of this rule. It might be *possible* for us all to learn and carry out the other rule; I think it would be more difficult than learning to spell "ghost" wrongly. But in the case of the unorthodox rule, whatever we *might* do, in fact it is not followed.

Necessary Propositions of Logic.

A child might see at the door of a cinema the words : "Only children accompanied by adults admitted." If he had taken the precaution of bringing a parent with him, he would infer, and correctly, that he would be allowed inside. *Correctly*, because that is what the words mean here. Are we also to say that the child's inference is correct because it conforms to the necessary truths of formal logic ? What would be the principle of formal logic to which this inference conforms ? Would it be that "Only S is P" implies "All S is P" ? This is certainly a proper rule of interpretation in this instance, but does "Only ignorant people believe this paper" imply that "All ignorant people do so" ? Are not the same sentence-forms, the same syntactical devices, used to indicate quite different inferential procedures in different contexts ? Are there valid rules about what *any sentence of a certain syntactical form* means or implies ? Fortunately, we have some evidence on this question, that is to say, the work of the formal logicians from Aristotle to the present day. They must by now either have overcome, or have abundantly illustrated, the difficulties of this project.

I shall try to summarise the traditional method :

(1) A proposition is identified by a sentence ; it is what that sentence, however and whenever used, means. *E.g.*, "Only man is vile."

(2) Sentences are sorted into types solely by virtue of their syntactical form : a given form is identified by an expression in which variables replace all non-syntactical features. *E.g.*, "Only S is P."

(3) A set of standard forms is selected and rules are formulated to show the inferential relationships between all the members of the standard set. *E.g.*, the Square of Opposition, which is happily designed to help students to memorise these rules of inference. But, of course, the Square is not a complete set for the task undertaken ; we have to add, not only the converses, inverses, etc., but also such sentences as

"All S is P and all P is S," "All S is P or all P is S," etc.

The rules say what the logician says shall be implied by "All S is P," etc., and this is always done by giving another member of the standard set, whether as an equivalent or as a subaltern. *E.g.*, "All S is P implies some S is P."

(4) So far as the standard sentences go, logic is formal, *i.e.*, inferential relationships are to be read *from the form of the sentence*. In other words, I infer "Some men are mortal" from "All men are mortal," because I recognize these to be of the form "All S is P" and "Some S is P," and recall and apply the rule that connects these two.

(5) As for all kinds of sentences not included in the standard set, these are to be "expressed in strict logical form," *i.e.*, are to be translated into one or other of the standard forms, and all further inferences made according to the rules formulated. But for the actual translation, it would fortunately be impossible to formulate rules. As a recent writer says :

"Re-phrasing is almost always necessary before the rules of symbolic logic are applicable to a statement and . . . it is permitted on a purely intuitive basis until a standard form is obtained ; After this, all further translations must be covered by a rule."*

This might seem to leave it to us to decide, on a given occasion, what is meant by "Only man is vile" or "Students only admitted." But our freedom is limited by the fact that the relations of any one expression of the Set of Standard Forms to all the others, is controlled by certain very explicit rules. So that, while I might be inclined to translate "Only griffins breathe fire" into the standard sentence "Nothing that is not a griffin breathes fire and all griffins do breathe fire,"

* John Cooley, *A Primer of Formal Logic*, p. 334. This is the best discussion I know of standard forms to be found in a text-book.

I should hesitate about the second clause, because it will by rule imply "Some griffins breathe fire," and is this what one would really mean by the original sentence?

(6) What the method must insist on is that a *standard* form has one and only one interpretation. Thus, in the Standard Language introduced in *Principia*, "If he is the thief, then he will have the missing jewels, and by jove he *has* the missing jewels," does not imply "He is the thief." For the "If—then" is a standard idiom and we are not to say that "If p then q implies "If q then p ." So that the very common use which equates "If p then q " with "If and only if p then q " must be regarded as a violation of the rules. Let the non-standard forms vary as they will; translation from them is "permitted on a purely intuitive basis"; but translations from a standard form are restricted by rule.*

It appears, then, that the formulation of rules of formal inference is possible only upon two assumptions:

(A) We are prepared to take a sentence out of its context and to say what it means. This gives us a regular correlation of a sentence with other sentences which it implies or is implied by. According to this procedure, I might say that "All those present are under 40" implies "There are some people present under 40," but not that "All griffins breathe fire" implies "Some griffins breathe fire." I claim to be able to make inferences from one sentence (abstracted from ordinary discourse) to others; but not to be able to do so merely in virtue of their syntactical form. Even this more modest claim cannot always be carried out; very often an abstracted sentence will simply be indeterminate as to what it does imply, allow or exclude.

* Tarski, in his *Introduction to Logic* gives the passing tribute of a sigh to this use of "If—then" by the mathematicians, p. 28.

(B) We can select a set of Standard Sentences, each one distinguished solely on account of its syntactical form ; and determine by rule the logical interrelations of all members of the set ; and the set must be such that any sentence selected under (A) can be translated into a sentence or system of sentences from the Standard Set. When this has been done, its logical relationship to any other corresponding sentences in the Standard language will be *obvious*—i.e., can be read from the syntactical form.

I have been discussing the traditional method. The new symbolic logic of *Principia Mathematica* is *formal* in just the same sense as the traditional logic, i.e., we are given a standard set of expressions into which all ordinary discourse is to be translated, and we are given rules as to how these expressions are to be manipulated in relation to each other. What is new in the modern logics is the axiomatic or postulational method. The rules are now conveyed or taught by exhibiting the standard expressions and explaining how to use them (1) by the postulates, (2) by the theoretical introduction and commentary, and (3) by the many examples given as theorems.

Since that time, formal logicians have shown great misgivings about the invaluable introduction and commentary to be found in *Principia* alongside the postulates and theorems. This may arise in part from the natural vanity of an expert who has invented a new language and *thinks* it ought to be good for all possible uses ; but in part it arises from serious theoretical considerations.

First, the comment is written in good English discourse, and there are not (and, as I think, will never be) any set of formulated rules for the interpretation of ordinary discourse. Would it not be possible, logicians have asked, to replace the comment by something briefer and more exact, written in a language whose rules *could* be formulated, i.e., a language in which all inferences are made by applying written rules to standard expressions ? This might seem to avoid all possibility that anyone would *misinterpret* the comment, and so fail to learn the rules of inference from the postulates and theorems of *Principia*.

Second, the use of natural prose in the comment and explanation, with the introduction of such homely phrases as "true," "false," "proposition" and "implication," shows only too plainly that the authors of *Principia Mathematica* obtained their knowledge of logic elsewhere. In other words, they had to *know* how we do in fact make inferences before they could set down rules for making inferences from standard expressions. And they seem to presuppose that the reader knows this, too, and will check their theorems by his own previous logical knowledge. This (as I suggested in my introduction) makes it look as though the postulates are *descriptive*; that they are trying to describe a structure which already exists (in some sense), and that such description can be tested only by someone who is already acquainted with this structure. It has therefore been suggested by some who hold that logic is conventional, that it ought to be possible to draw up a set of rules which would not presuppose any such knowledge in the reader—which could teach logic to someone entirely ignorant of our modes of inference. And a set of rules, also, which could not be criticised on the basis of any previous logical knowledge, simply because they would be, or could be, what we *mean* by "logic." Could we not have a code of rules so *complete* that nothing is left to that sort of "intuition" which a person is able to exercise when he interprets the written rules of a game he already knows how to play?

With regard to the first objection, there is no reason why we should not invent a set of standard expressions in which to convey the explanations and directions necessary for the teaching or showing of the rules of inference. We can then formulate the rules for this "metalanguage" if we like. But we have no reason to suppose that the new set of expressions will not be misinterpreted. For if they are not already well understood by the reader, then *their* use has to be explained in some other language; and if, on the contrary, we want a language which is already familiar, what is wrong with the English we actually have in *Principia*? The only symbolism we can be (fairly) sure of

understanding without being given any rules of application and interpretation is not a symbolism which is inherently "clear" "self-evident," "unmistakable," but just a symbolism with which we are already perfectly *familiar*.

Could we do without the explanation, and trust to the postulates and theorems to teach the rules of inference? One is inclined at first to say no. I may see that

$$(p \cdot p \supset q) \supset q$$

and see many other theorems which have a similar pattern; but how shall I learn that wherever I can assert the conjunction of an implication and its antecedent, I can go on to assert the consequent? What the postulates and theorems give us are particular examples; they are given to help us to apply certain rules *to new cases* not given as examples. But unless we have rules as well as examples, and unless we make some distinction between the two, "How can we know the dancer from the dance"? But my main point is that the dancers, the pictures, are there to show us the procedure. If a person were already familiar with a very similar procedure, he might gather the way to make the moves from examining the examples alone—as (I suppose) is done in decoding enemy messages. But why not give the explanation as well as the examples?

As for the second objection, I think it really asks that we should try to draw up a set of rules of inference in which no step has to be taken except in accordance with a special rule. In his paper "Truth by Convention," Professor Quine shows that this cannot be done.* We cannot, he says, formulate postulates which assign truth to propositions of the "If *p* then *q*" form *one by one*, because there is no limit to their number. We must have postulates which assign truth to classes of propositions identified by certain properties—in fact, by their syntactical form. But what is to tell us that a given sentence *is* a sentence of a certain form? We might then ask for a postulate which lays this down; but this postulate is also general and has to be

* Philosophical Essays for A. N. Whitehead. Longmans. 1936.

applied ; What is the rule for *its* application ? We are in a vicious infinite regress, like that described by Lewis Carroll in "What Achilles said to the Tortoise." Indeed, this very same regress also faces us when we try to make inferences by the *modus ponens*. The rule or postulate says that if you have p and $p \supset q$, you may assert q ; can we also ask for a rule which tells us what to do when we come to apply this postulate. This would have to take the form :

If you have $(p \cdot p \supset q)$ and $((p \cdot p \supset q) \supset q)$ you may assert q . But I should then go on to ask for a postulate to tell me how to apply the postulate I have just written. And so on. .

These insatiable demands seem to me to rest on a misapprehension of the nature of a postulate. A postulate is given to illustrate a rule, and to teach it by illustration. We may, if we like, regard it as itself the expression of a rule, or we may formulate our rules as "Rules of Inference" alongside the postulates. What we cannot do is to refuse to use the postulates as rules and at the same time to try to remedy the resultant *stasis* by asking for more pictures, *i.e.*, more postulates. We cannot ask for a rule for the application of a given rule to every single case. For the whole nature of every rule we offer will be that it is something to be applied in new cases. The question whether somebody who has read the code can or cannot apply the rules, is the same as the question whether he has understood the rules as the code formulates them. They have been written in order to teach him a procedure : we find out whether he has learned the procedure or not by seeing what results he gets when he applies it in new cases. The notion that all possible applications of the rule can somehow be included in the formula confuses a rule with a *statement or summary*.

This does not mean that we shall not be helped by more detailed examples ; the working out of particular theorems offers us such examples. But there can be no set of formulæ which together guarantee that anybody who has read them will be able to apply the rules. For every theorem is itself a rule—of more limited use than the more general rule because it has a more restricted application.

It has itself to be applied to new cases and may be applied wrongly. And we cannot work out all the possible theorems even for a set of standard forms.*

There is another comment to be made on the second objection—on the point that a logician must know the rules before he formulates them. In a trivial sense this would be true of a person setting out an entirely new calculus : first he thinks out the “pieces” and the “moves,” and then he sets out the rules. The procedure comes first and the formulation afterwards. But this is true in a much more important sense of a code for formal inference. For here the aim is to write out rules for procedure which we do actually use in making inferences between the members of the set of standard forms.

There must, therefore, always be an “intuitive” test of the theorems of a code of formal logic. I have made the suggestion that this test, if applied carefully, will show that these codes very considerably over-simplify the procedures we actually employ ; but we could say that the test gives a *positive* result, if we recognize in the rule offered in the code, a formulation of a procedure which we should very commonly apply. Thus if I say : “All these pennies are George Vs,” and am later asked, “Did you state that some of those pennies were George Vs” ? I might very well agree that I *had* stated it—although I *might* say, “No, I said that they *all* were.” The code restricts us to one use of “all” and “some” ; but it is a use which we all recognise as current.

As I have tried to explain, the notion of necessary truth seems to me to presuppose rules for procedures upon which we not only might, but actually do, reach agreed results. It is not enough to say that the necessary propositions we have been considering are expressions (in special form) of rules. So are the rules of bad spelling and bad division, and the rules of any uninterrupted calculus. Nor is it

* I do not wish to give the impression that I think logic books work out theorems only as examples to help us in learning the rules ; that one learns no new truths from these books. That would make the patient reader—“one that goes to sea For nothing but to make him sick.”

enough to show that they are expressions of rules which we actually follow : so are the rules of spelling, grammar and chess. The rules we are discussing are distinguished by a peculiar use ; they are actually employed in making inferences from, and to, empirical statements, as well as in pure deduction or calculation. This brings me to the most difficult question. Could it have happened that we had different rules of inference ? If we abstract for a moment from the use of mathematics and logic in making inferences from, and to, *empirical propositions*, then the question I have just asked would seem to most people (I think) to allow the answer Yes : Why not conduct these pure calculations differently ? (Why not spell differently ? Why not play chess differently ?) But many people feel that, if we were to do arithmetic and to make formal inferences differently, then we could not expect to be able to use these modes of calculation in the making of inferences *about the world*.

Alternatives to the Necessary Truths.

It would plainly be a misuse of the word " convention " to claim that necessary truths are " true by convention " and yet that no alternatives to these truths could have been adopted. I have claimed that necessary truths are expressions of rules ; that it is an empirical fact that we have adopted these rules ; and that it is an empirical fact that in following these rules, we all reach conclusive agreement ; and that this agreement is the only test as to whether the rules have been applied correctly. This is a conventionalist interpretation of necessary truth, and does certainly allow the possibility of alternatives. The question " Is this or that inference valid ? " must refer us to a rule and asks for an adjudication, not an empirical statement. But the question " What rule do we follow here ? " either is, or (where one rule depends upon another) will lead us back to, an empirical question—one whose answer *might have been* different.

What rule do we apply to the numbers assigned by counting and measurement ? I have not challenged the view that it is possible to codify the procedures which we

actually follow here. I have assumed that our use of the numerals is strictly uniform, in accordance with the arithmetical calculus. What I have done is to try to show the conditions which permit us to employ this calculus in this way.

(1) We have procedures of counting and of measuring and these can be taught in such a way that those who carry them out in the same circumstances regularly reach the same results.

(2) We have procedures of pure calculation upon these numbers and here again we are all able to learn them and to reach agreement in our answers to the same problems.

(3) Where we do not reach agreement in counting or in measurement, we can, as a general rule, find independent grounds for saying either that the system has been unstable or that we have misapplied our procedures.

I do not think that there is anything in these conditions to confine us to any one calculus of arithmetic.

What rules do we apply in passing from a statement of one syntactical form to corresponding statements of other syntactical forms? I have tried to show that it is no simple matter to formulate these rules. What a sentence means (what follows from it) is something to be gathered step by step from the whole context of utterance, and it is a familiar fact that we make use of one syntactical form to convey different meanings.

It might be argued that these variations do not introduce us to alternative logics, but only to alternative notations for use with the same logic. This argument would hold that it is up to us to examine a statement from ordinary discourse, and to discover, step by step, from the whole context, what the words mean *here*; then to translate the expression into a language (whether newly invented or a selection from current language) whose syntactical forms are used in strict conformity with written rules. Further inferences are then made by these rules. And, the argument would hold,

the logic of the standard language would be the classical logic.

I feel sure that no such standard and omniscient language has ever been invented, and I feel great doubt as to whether it could be. For it seems to me that we do make statements which can in certain cases be determined as true, and, in certain other cases, as false, but which, in certain cases, cannot be determined as either. This is the thesis put forward by Dr. Waismann in Parts II to V of his latest paper to the Aristotelian Society, and I find that part of his paper convincing.*

If this view is correct, we can find traces of genuinely alternative logics in the procedures of ordinary discourse. And the essential point here is that we are not dealing with an eccentric or unorthodox system whose possible applications are yet to be discovered. For the statements which employ this "new system" are certainly *verifiable by experience* in certain circumstances. The *use* is known. It will, however, also be instructive to consider invented variations; e.g., languages in which there is no form of negation at all, or in which there are no general statements at all, or no inference by *modus ponens*.

Concerning such alternative logics (as concerning alternative arithmetics or modes of counting or measurement), we can ask the following questions:

(1) Are the rules such that they can be taught, so that it will be possible for people to apply them and to reach agreement in the results of applying them? The usual method of introducing a new procedure is to explain it by analogy with procedures we actually follow; but a set of new procedures might be so novel and so complex that they could not be taught.

(2) Could they be used as modes of inference in ordinary discourse? This is a difficult question, for, of course, the meaning of "empirical statement" is

* *Proc. Arist. Soc.*, 1945-6, pp. 77-86. I think that the middle of the second truth-table on p. 84 has been misprinted and should read "F. T. F."

determined by the inferential procedures admitted. If a given procedure were adopted, could we regard the result as sufficiently like our ordinary empirical discourse, to be called "a kind of empirical discourse"?

Here, again, the fundamental thing must not be lost sight of: How a given rule or set of rules is applied depends upon the agreement of those who are applying it. Should we say that a statement which was quite indeterminable in certain circumstances was "an empirical statement," or that it was in open conflict with our notion of "an empirical statement?" We might decide either way, whatever the logic books say; and, in fact, as Dr. Waismann's examples show, there are instances of such statements which we do all in fact call "statements" and "empirical statements." This (it might be argued) shows that we recognise them as useful for describing the world, or corners of it. And I see no reason to think that there are not many other modes of discourse, unfamiliar to us, which would also serve the same purpose.

II.—By J. O. URMSON.

ONE of the points which Mr. Britton has made in his paper is that if we consider carefully the way we use necessary truths, we shall find that it is such that it would be more appropriate to call them rules of procedure than to call them truths, although, of course, to call them truths is perfectly correct usage. This is not new, as Mr. Britton is well aware ; he himself points out that Louis Carroll hinted as much. I suppose also that traditional logicians who refused to use the dodge of regarding the principle of a relational argument as being the major premiss in order to reduce such arguments to syllogistic form, were thinking somewhat along these lines.*

Without binding myself to accept the letter of Mr. Britton's remarks on this subject I none-the-less hold that in general what Mr. Britton says about it is true and worth saying.

But Mr. Britton obviously wishes to connect this point with his other view which he states in such words as "all necessary truths are true by convention." Louis Carroll and the traditional logicians certainly were not hinting at such a view as this. I doubt if any of them had even considered it, and if they had, I think they would have been shocked. The point is perhaps worth noting ; for it does not seem that the view that, for example, logical truths function as rules of inference rather than as statements of fact need involve holding that we could have used alternative rules of inference. Perhaps Mr. Britton does not himself think that this is involved, for he says "I have claimed that necessary truths are expressions of rules ; that it is an empirical fact that we have adopted these rules and that it is an empirical fact that in following these rules we all reach conclusive agreement ; and that this agreement

* e.g. Joseph Introduction to Logic, second edition, page 295 :

"The axiom, therefore, is not one of the premisses from which we reason when we argue that ' $a = b$ and $b = c$, therefore $a = c$ ' it is the principle in accordance with which we reason."

is the only test as to whether rules have been applied correctly. This is a conventionalist interpretation of necessary truth and does certainly allow the possibility of alternatives." Thus we see that, in Mr. Britton's view as here expressed, it is not merely the fact that necessary truths are expressions of rules, but this fact combined with two others, which shows that necessary truths are true by convention. What of these two other points?

When Mr. Britton says: "It is an empirical fact that we have adopted these rules" (*i.e.*, necessary truths) his statement is simply false. Certain bodies of people such as the M.C.C. and the Committee of the Portland Club have no doubt gone through the process of adopting certain rules, but neither when learning to argue and reason, nor at any later date has Mr. Britton or any one else gone through the process of adopting any rules. Some people have said that in asserting necessary truths we are recording our determination to use words in a certain way. Such a formulation suffers from a kindred defect. Of very few words indeed is it true in any natural sense of "decide," that my use of them is governed by a decision to use them in the way that I do. But perhaps even if this point be admitted to be well taken, it will be said that it is merely pedantic, as people who believe that political obligation is contractual feel when they are told that there is in fact no making of a contract. But that we adopt, choose, or single out the rules we use, even if the adoption, choice, or singling out, is admitted to be an historical myth, is the point at issue and not a datum. Further, Mr. Britton has brought arguments to show that we find out necessary truths by observing people's procedure, *i.e.*, that it is an empirical fact that these rules are used; but I cannot find any which suggest that it is an empirical fact that we select these rules from amongst alternatives.

Perhaps, then, Mr. Britton will allow me to state his empirical fact not as the adoption of certain rules but as the use of them. It is an empirical fact that we use these rules, let us say. Well, I will grant this and grant that there must therefore be a conceivable alternative to our using them,

that we might not have used them. Let us agree that it is an empirical fact that ducks do not use them ; then it is conceivable that we might have resembled ducks in not using these rules of procedure, not by the device of using other rules, but by using none at all. The conceivable alternatives might also include the using of other rules, I think they do. But it certainly does not *follow* from what Mr. Britton says, so far as we have considered yet, that there might be some alternative rules.

Perhaps then the third part of the sentence I have quoted from Mr. Britton will fill the gap. Let me quote this part again : " It is an empirical fact that in following these rules we all reach conclusive agreement ; and that this agreement is the only test as to whether the rules have been applied correctly." Alongside of this summing up we must of course remember the examples and arguments with which Mr. Britton originally made his point. Mr. Britton's first example, so far as this applies to mathematics, is of accountants doing addition. Now from my experience of adding figures, and particularly money, as is usually the case with accountants, the most natural test of correctness is whether the answer is what you expected. If you pay one hundred men, say, and add up the payments to see how much you have paid, the most obvious test of whether you have added correctly is to see whether you have got the amount of money left from what you started with that your addition says you should have left. It is only when this test fails and you get an answer different from what you expect that you check the figures with someone else. Checking with other people is of course an important test, but I can imagine cases where it is of little importance. If from agreed data a number of people got one answer to the mathematical problem, say, of how strong a piston needed to be, and one got a different answer from the majority, and if pistons were made to both specifications and the piston made to the majority's specification broke and the other did not, then, may be, I would say " This man is the only one who can do this differentiation correctly." Of course, if very many more people got the

answer which demanded the fragile piston, I might come to suspect that they were right and perhaps the agreed data from which they had worked were faulty, so that the man who specified the correct piston did so by a lucky chance. But then again, if the agreed data were checked and re-checked and found to be correct, then once again I might say that it was not a fluke after all, but superior mathematical genius.

So I would first say that agreement of calculators is not the only empirical test : there are other, though not necessarily better, empirical tests. Sometimes, too, without making any other empirical tests, we will deliberately set ourselves in the minority of one against the agreed opinion of, in Mr. Britton's phrase, "instructed people." The first man who saw that Euclid's method of constructing an equilateral triangle involved unjustifiable assumptions and was not therefore in accordance with the rules of procedure of geometry, was presumably not worried by so disagreeing with the results of all the other instructed people who had considered the problem. I expect he revelled in this disagreement. No doubt he did expect that when he had pointed out why, all instructed people in the future would see that all instructed people in the past had been wrong, but surely his certainty of the past mistake was not based on expected future agreement? Maybe Mr. Britton can show me that I have failed to understand him here, or will be able to restate his point without essential modification, but so as to avoid my objection. But at present it seems to me, if immodest, yet intelligible for a man to claim that his result after carrying out a chain of reasoning is right and all the rest of the experts are wrong, merely as a result of looking at the problem and without empirical tests, and further it seems to me that when we check our reasoning by outside reference, agreement with informed opinion is merely one important, and not the only, criterion.

I conclude from this discussion, I hope not too hastily, that Mr. Britton's discussion of the way we use necessary truths, interesting and valuable as it is for its own sake, is independent of and does not imply the view that we could

have used alternative necessary truths. And that there should be a possibility of having alternatives is at least implied by the doctrine that necessary truths are true by convention, as is clear from Mr. Britton's paper.

But is the view that we could have alternative necessary truths only *part* of what the conventionalists, including Mr. Britton, want to say, or is it *all* of what they wish to say? I am not sure. If it is all they that wish to say, then in the case of some necessary truths their contention is obviously correct, because we have alternatives already. We have several alternative geometries, and commutative, and non-commutative algebra, that is to say, there is an algebra in which " $A \times B = B \times A$ " is a necessary truth, and an algebra in which it is not. In logic, too, as Dr. Waismann and Mr. Britton point out, *modus tollendo ponens*, the inference from an A to I or E to O proposition, is not always treated as valid and even some more radical wavering in our application of the laws of the logic of standard forms can be observed.

But apart from the question whether alternatives are possible to all necessary truths, I think conventionalists wish to do more than say there may be alternatives. I think they are doing something which may be called explaining the nature of logical necessity. Necessity, as it arises in sentences with words like "must" and "can't" has worried empirically minded philosophers. If I may be permitted a crude and inadequate epigram, the "can't" of empirical matters of fact is explained by empiricists as really being a "don't." "Tigers can't fly" becomes "Tigers don't ever, as a matter of fact, fly." The conventionalists are performing a parallel service for the logical "can't" by translating it by "won't." "What is green can't be blue" becomes "We won't call the same thing 'green' and 'blue'." Necessary truths are man-made and the reason why we cannot be forced in any possible circumstances ever to recognize their falsity is precisely because their truth or falsity, or perhaps their use or abandonment, is in our power.

I should like to know whether this does, however inadequately, represent the conventionalist contention. If it does,

then conventionalism seems a monstrously difficult theory to maintain. For now, beyond the question whether, to confine ourselves to logic there are alternative logics, we have the further question :—Why have any laws of logic at all? Mr. Britton states a criterion for a genuinely alternative logic as being “if a given procedure were adopted, could we regard the result as sufficiently like our ordinary empirical discourse to be called a kind of empirical discourse?” I want then to ask, “If no rules of procedure were adopted, could we regard the result as sufficiently like our ordinary empirical discourse to be called a kind of empirical discourse?” And if we could not, what kind of “cannot” is this? Is there just an empirical difficulty in the way of doing without laws of logic in our discourse? Or is this a second order convention not to do without first order conventions? If it is a second order convention, could we not change this second order convention and decide to recognize a mode of behaviour without laws of logic, without rules of inference, as a kind of empirical discourse? Or have we here a genuinely logical “can’t,” which insists on meaning “can’t” and refuses to mean either “do not” or “will not”?

I think we shall be able to begin to answer these questions if we consider further a remark of Mr. Britton which I have just quoted. I shall quote more fully this time. As a criterion for alternatives to necessary truths, to discover whether they are satisfactory, Mr. Britton suggests two tests of which the second is as follows :—

“Could they be used as modes of inference in ordinary discourse? This is a difficult question, for of course the meaning of ‘empirical statement’ is determined by the inferential procedure admitted. If a given procedure were adopted, could we regard the result as sufficiently like our ordinary empirical discourse to be called ‘a kind of empirical discourse’?”

Now what does Mr. Britton mean here by “sufficiently like our ordinary empirical discourse”? Clearly sounding somewhat similar or being capable of being written down and so on, are similarities, but not of the kind in which

Mr. Britton is interested. He tells us the answer, I think, a few lines later on when he says "I see no reason to think that there are not many other modes of discourse unfamiliar to us which would also serve the same purpose." The test of similarity, then, is fulfilling the same purpose. Therefore the question we have at present on hand is "Would a mode of behaviour, carried on in accordance with no rules, fulfil the same purposes as modes of behaviour carried on according to rules, and in particular, the same purpose as is fulfilled by ordinary language with its ordinary rules of inference?" If the answer is in the negative, then there is at least one case of a "must" and "can't" which is not amenable to a conventionalist analysis, and, instead of having a rule of thumb for translating modal adverbs, we shall have to treat each as we find it.

We must beware here of still being muddled as to the exact question we are asking. For the question "Would A fulfil the same purpose as B?" is itself an ambiguous question. We are asking "Would a mode of behaviour carried on in accordance with no rules fulfil the same purpose as ordinary language?" Compare that with the gardening question, "Would a fruit spray with no arsenic content fulfil the same purpose as ordinary arsenic spray?" At first sight that is very similar to our question, for it would be decided in the affirmative by someone producing a spray which did deal as efficiently with codling pests as arsenic; and our question would appear to be capable of being given an answer by empirical means in the affirmative by, say, a couple of people indulging in a ruleless behaviour which enabled information to be given etc. as well as the King's English. But our question is really different because, unlike the gardening question, it is one which we feel might be answerable in the negative by non-empirical means, and even capable of being answered in the affirmative without our even having a hope that any one would ever actually produce an example. In this respect it is like the geometrical question, "Could we prove such and such a theorem without using such and such an axiom?" which is capable of being given an affirmative answer by someone

producing such a proof, but which can be answered conclusively by other means.

Compare our question now with the question, "Would a wedding without a religious ceremony fulfil the same purpose as one with a religious ceremony?" In a suitable context, factual doubt here might have shrunk to vanishing point. The doubt could perhaps best be described as a doubt whether the two types of wedding could be appropriately described as fulfilling the same purpose. A verbal decision is required. Now we might be faced with a decision whether a mode of behaviour could be described as fulfilling the same purpose as ordinary language from this angle. But that is not the question we are faced with as yet. At present we need only ask whether a mode of behaviour without rules could resemble language sufficiently for this point of linguistic doubt to arise. If we find it can, only then need we go on to worry about the linguistic doubt.

If you go on to ask "What kind of question is ours, then?" I can only say that we are asking a question of the *logical possibility* of a ruleless behaviour being a possible substitute for our more or less regulated language. Practical impossibility is one thing, linguistic impropriety another, and logical impossibility another. I see no possibility of, and feel no desire for the reduction of any of these to either of the other two. The position I want to adopt is this. In the case of many logical "can'ts", the conventionalists are basically right in the sense that we might well have found ourselves using a different rule from the rules we in fact use. I will also agree with them that this is not merely an academic possibility, but that we do find actual variation in the rules we use. Mr. Britton gives examples of such variation and Dr. Waismann many more in the paper referred to by Mr. Britton. All this is very well worth pointing out, but I can see no reason *a priori* why all the rules of logic should be conventional in this sense. In particular there seems to me to be a second order logical rule; that any language must have some rules, and this seems to me not to be conventional in the required sense. So I would say that it is logically necessary in a non-conven-

tionalist sense that any language, or any mode of behaviour which is to fulfil the same purpose as language must have some rules. I am prepared to find that a set of languages could be constructed, each of which has no rules in common with any other ; I am prepared to find that there are some rules which any language must have, and also a sphere where alternative rules are possible. I am not prepared to find that there is one and only one possible logic—though it would be nice if there were—because, as I have said, I think that it has already been made clear that alternatives can already be found in our present language. So I am a Trimmer ; I cannot agree with the absolutists, though I am not called upon to answer them now, and I cannot go the whole way with the conventionalists in reducing all cases of logical necessity to decisions.

I propose now to consider further my second order logical rule because one thing which I am trying to establish against Mr. Britton is that there is at least one case of logical necessity not being reducible to convention by his technique. And my case is a logical necessity which must have been so clear to Mr. Britton himself that, while he asks the question “Could we have alternative logics ?” he never even asks “Need we have one at all ?”

Let us commence our survey of the necessity of having rules by examining words. With certain exceptions, words have what I shall call an area of reference. Saying that they have an area and not a point of reference is merely a shortway of saying that they have the characteristics, in varying degrees of vagueness and generality. But the area of reference of words is limited, by which I do not mean that we can precisely limit the area—draw a line round the penumbra, to borrow Professor Wittgenstein’s apt illustration. I merely mean that we can find things to which the word does not apply. The vagueness and generality of the word “chair” do not prevent me from knowing that I cannot call a curtain by that name. Where the area of reference of a word ceases to be limited, the word ceases to have a meaning. Therefore I cannot produce an example whose vagueness is not limited. But we can observe words

proceeding towards the limit of limitless vagueness and generalities. For example, we can observe the word "stupendous" as used in the film world, where the phrase "Stupendous new film" means roughly the same as "New film." Similarly in the language of estate agents, the phrase "desirable freehold residence" is used in such a way that only the initiated few can distinguish a merely freehold residence from a desirable one. Even the emotive as well as the literal significance of such words has been well nigh lost.

However, the limiting of the vagueness of words does not depend merely on the restraint and modesty of cinema magnates and estate agents. We have other devices, some practical, some logical, for ensuring this. For example, we limit the number of shades of colour which we may call red, partly by considerations which are not logical; but also we preserve and limit the meaning of "green" by avoiding the use of it where we used "yellow" or "blue" or some other colour word. Not being good at colours, sometimes I do not know whether to call something say "pink" or "mauve." I do not mind which I call it, I shall probably be wrong anyway, but I do know that I must not say that it is both pink and mauve.

It is then, if you like, arbitrary from a logical point of view where we draw the line between two colours. It is arbitrary, perhaps, that we do not allow that one is a species of the other. To know that a thing cannot be both pink and mauve all over does not involve necessarily great insight into and a sensitive discrimination of these colours. For all that, it would be very misleading to call the sentence "a thing cannot be both pink and mauve all over" an arbitrary rule. With all the arbitrariness which I have admitted, there remains the fact that without some such rules, we could not talk about colours at all because we should have lost our means of limiting the vagueness of these words. In general such logical rules as the rules of classification and division may be regarded as devices for limiting the area of reference of words, without which they would lose their meaning. While allowing the arbitrary character of many of our

classifications of species, while allowing that there may be languages in which the limiting of the area of reference of such words as colour words is done by a set of logical rules which does not include our laws of classification and division, the fact remains that if a language is to serve the purposes for which we do use language, the words contained in it must have their area of reference limited by some devices or rules analogous in their effects to the classificatory machinery by which we limit the vagueness of class names. This is not just a convention ; nor on the other hand is it some super-intuition of the nature of the world, a cast-iron pre- and retro-vision which cannot be wrong. It is seeing that we must do something to achieve certain purposes if we wish to achieve them.

Let us now consider sentences. I have introduced this short discussion of words with its conclusion that whatever arbitrariness there may be in the choice of the actual rules of a language for limiting the area of reference of words, there must be some rules for limiting this area—a non-conventional rule that there must be some conventional rules—for the sake of analogy. I want to compare words and their area of reference, limited by rules such as the laws of division and classification, with sentences and their logical rules. And I wish to compare the law of contradiction with the non-conventional rule that there must be other rules of words. As the area of reference of a word must be limited, so the possible application of sentences must be limited, and this is what the law of contradiction says as I intend to interpret it.

Consider a statement like “some of us are bald.” That statement does not pinpoint any one state of affairs. This is partly due to the vagueness of the words it contains, “bald” is a notorious and useful lecture-room example. It is also partly due to the compatibility of sentences of the form “some X is Y” with “20% of X is Y” “30% of X is Y” and so on. But this area of reference is bounded by logical rules ; not clearly bounded, for we are not at all certain whether “some X is Y” excludes “100% of X is Y” ; but it does exclude “0% of X is Y” ; if it did not,

then, no matter how we tighten up the meaning of "bald" and other words, the sentence "some X is Y," "some of us are bald" would be useless. Perhaps it does not matter very much how we limit the area of reference "some X is Y," just what rules of immediate inference from it we follow. But some such rules there must be, or we should have no use for the sentence at all.

The same point can be made in the case of complex statements of the form "p or q." Once again, here, many situations will satisfy such a statement, but logical rules limit the situations which will satisfy it, though once again we waver as to which situations do satisfy it. Does "p and q" satisfy it? But I do not think that I need to labour this point, because I am only pointing to the reverse side of a well-known medal. Time and time again we are told that the immediate inferences from a statement do not follow from its meaning; finding out its meaning is finding out the possible immediate inferences from it. I am pointing to the connected fact that unless the statement excluded something, it would be useless and have no meaning at all. It would be useless to say "some X is Y" if this statement were compatible with all percentages of Xs being Y from 0 to 100 inclusive. It would be useless to say "p" unless that excluded "not-p."

But we shall be told that we might have a multi-valued logic in which the simple dichotomy "p or not-p" does not arise. Perhaps we might, so let us have a generalized law of contradiction. Let us say "In an n-valued logic no proposition may have more than n-1 true values." This gives us, for a two-valued logic, the normal law of contradiction, "In a two-valued logic no proposition may have more than one truth-value." It does seem to me that this must be true of any logic which is the logic of a language serving the same purpose as ordinary empirical discourse. Without it the area of reference of a statement is unlimited and no meaning can be attached to it.

It does not seem to me that to claim to know that my generalised law of contradiction must be a law, or must be implied by the laws of any possible logic, is really very

presumptuous. It is not to claim the kind of thing which has worried philosophers, such as claiming to know that every event has a cause, nor to claim to know what the world is like without looking at it. I am claiming that to do a certain kind of thing, the kind of thing which is done, as Mr. Britton says, in ordinary empirical discourse, I must follow a certain course of procedure. And this is a logical "must"; it is not like saying that to serve good boiled potatoes you must put salt with them.

Mr. Britton is right in saying that it is merely an empirical fact that people use the logical rules which they do use. It is merely an empirical fact that people use rules of logic at all. From this it follows that a world with no logical rules at all is conceivable. But from this a conventionalist conclusion does not follow. It does not follow because it is also just an empirical fact that people communicate and use language; but the fact that we could not do the communicating without rules does not seem to me to be just an empirical fact.

A number of analogies can be found. But take first an analogy from maps. It is just an empirical fact that people use maps. They need not. They certainly can and do use maps with many different projections. But a map would not be a map if it had no projection. It is not merely that we would not call it a map. We could not fulfil the purpose for which we use maps without a projection. When I say that no map may consist of a monochromatic plane surface, that is not because I am determined so to use the word map. It is because a monochromatic plane surface would be of no use in finding one's way about, and so would not fulfil the purpose in view. Again, consider the fact that the rules of a gambling game must permit of one side winning. You could not have, say, the rule that the game shall consist of an even number of deals or bouts, each player starting in turn, and that the starter of each deal must win. And this is not merely because the word "gambling" is used in a way which prevents such rules; the trouble is that the purpose of gambling cannot be fulfilled with such rules. We could extend the meaning of "gambling" to include stunt-flying for high wages without

dealing death to the word. Its meaning would be killed by extending it to such a procedure as I have outlined.

Thus logical necessity can be, perhaps, best understood in the light of the notion of purpose. A general purpose can be fulfilled in many ways differing in detail. Which to adopt will be a matter of experiment and convenience. Hunger can be satisfied by many means—perhaps even a stone could be treated in such a way as to give nourishment. But not any activity fulfils any purpose. Logically there need be no eating because there need be no satisfaction of hunger. Logically there need be no laws of logic because we need not communicate information. If we made noises according to no logical laws, we might indeed fulfil some purpose, but not the purpose which language fulfils. Such extra-linguistic purposes are already very well fulfilled by babbling.

III. *By* WILLIAM KNEALE.

I ONCE heard an American philosopher say of a colleague : "He was an interesting man in discussion until he went to Cambridge for a sabbatical year. After that he got so he couldn't understand." Whether the change described by this remark was a loss or gain for philosophy I cannot say, but it was evidently annoying to my informant ; and this is not surprising, for philosophers are always at their most irritating when they say they cannot understand. It is therefore with some misgiving that I announce I have gotten so I cannot understand the question we have been set to discuss.

If words are to be taken in their ordinary senses, it seems obvious that necessary truths cannot be true by convention. In common speech the word "necessary" means much the same as "indispensable," "inevitable," "without any possible alternative." Consider, for example, its use in the opening statement of the Athanasian creed : "Whosoever will be saved : before all things it is necessary that he hold the Catholick Faith. Which Faith except every one do keep whole and undefiled : without doubt he shall perish everlastingly." Admittedly the phrase "necessary truth" belongs to philosophers' jargon rather than to common speech, but its origin is clear enough. Philosophers say that a proposition is a necessary truth if it is impossible that it should not be true, *i.e.*, if there is no possible alternative. But in common usage the phrase "by convention" always implies the possibility of an alternative. If someone tells me that it is only convention which makes black the colour of mourning in Europe, I understand him to mean that the connexion established in our civilization is not inevitable ; and I think it very good evidence for his assertion, when he goes on to tell of some people on the other side of the world who use scarlet where we should use black. It seems clear, therefore, that anyone who speaks of necessary truths as true by

convention is guilty of self-contradiction, unless one or more of the terms he uses has a special sense in this context. Taken according to ordinary usage, his statement means that truths without alternatives nevertheless have alternatives.

It is rather surprising that neither Mr. Britton nor Mr. Urmson has anything to say about the appearance of paradox in the question we have been set to discuss. Mr. Britton does not speak of alternatives at all until the end of his paper, but when he does, he boldly heads his section: "Alternatives to the Necessary Truths." Mr. Urmson, after insisting, quite rightly, that the thesis of this section is essential to the doctrine that necessary truths are true by convention, goes on to state that, for some necessary truths at least, there obviously are alternatives. I assume nevertheless that neither of them is guilty of the absurdity to which I have just referred, and so I must look for some special interpretation which makes the question debatable.

In the introduction to his paper Mr. Britton makes it clear that when he talks of necessary propositions as true by convention he is using words in a special way, although not in a way private to himself. Necessary propositions, he tells us, are not propositions in the ordinary sense, and they cannot be said to have truth in the ordinary sense. In other words, we have here a kind of metaphor which may be seriously misleading to persons who do not understand the complexity of their own language. What is commonly called the assertion of a necessary proposition is really one way of formulating (and teaching) a rule, a way which involves the construction of an illustrative sentence. For in practice the acceptance of a proposition as necessary is shown by "the adoption of certain procedures of inference or transition from one statement to another." Although (as Mr. Urmson says) the views expressed here by Mr. Britton do not involve the conclusion that all necessary truths are true by convention, they do at least allow him to put the conventionalist thesis in a new way. He can now say that, when he uses the paradoxical expression "alternatives to the necessary truths," he is referring only

to the possibility of adopting procedures or rules other than those we have adopted. And he does in fact follow this line of argument in the last section of his paper. Having insisted that our adoption of a certain set of rules is an empirical fact, he there draws the conclusion that we might have different rules.

When Mr. Britton speaks of a necessary truth, he means apparently a truism such as "If a person is a grandparent, then he is the parent of a parent." And when he speaks of a transformation rule or of a procedure for passing from one statement to another, he means apparently a rule of usage such as might be expressed in a sentence of the command form, *e.g.*, "For 'parent of a parent' write 'grandparent'." I think that the truistic character of truisms is to be explained by the rules of usage of the symbols which they involve, and that truisms are often uttered, as Mr. Britton says, for the purpose of drawing attention to rules of usage. But I do not think that the rules of usage of symbols are what Mr. Britton supposes them to be, nor yet that his account of the relation between truisms and rules of usage is enough to show why truisms are said to be necessarily true. Before examining the conventionalist thesis in the form which he has given to it, I must say something about these two points.

In the first place, rules of usage cannot properly be treated as positive commands to say this or that in certain circumstances. I hope that I use the word "grandparent" correctly, but I certainly do not observe a rule of writing (or saying) "grandparent" whenever I see (or hear) the words "parent of a parent." Why should I? What would be the value of conforming to such a rule? The rules which I do observe are rather such as might be expressed by sentences like "Do not say 'grandparent' where you are not allowed to say 'parent of a parent'." In short, all rules of usage which determine meaning are prohibitions. This is true not only of dictionary definitions like that just considered, but also of ostensive definitions. A child does not yet know how to use the word "red" unless he has learnt that he is not to apply it to green

things. And there is nothing mysterious in this restrictive character of linguistic rules. Nature provides us with a disposition to chatter freely: we make languages for ourselves by adopting rules which canalize that activity. Once this point has been realized, my second point, *i.e.*, that about truisms, can be made quite simply. A statement is truistic in a given language if, and only if, (a) it consists of symbols for which restrictive rules exist in that language and (b) all statements inconsistent with it are forbidden by the rules of that language. It is not surprising that such a sentence should be said to be *necessarily* true, for there is indeed no alternative to its truth. Knowledge of the rules enables us to certify this, apart from all reference to experience.

If this account of the matter (or something like this) is correct, what are we to say of the suggestion that there might be different rules of usage for our symbols? Taken in the most natural and obvious way, this remark is a platitude. Everyone now admits that linguistic rules are conventional—at least in a wide sense of that word which does not commit us to saying that they were adopted by a deliberate act, like the rules of cricket and bridge. We can suppose, for example, that the sound “green” might have been subjected by Englishmen to the same restrictive rules as now apply in English to the sound “scarlet,” and we can say that, if this had happened, the set of sounds “All green things are red” would have been a necessarily true sentence. But is this *all* that the conventionalists wish to maintain? When they tell us that necessary truths are true by convention, they speak with an air of conscious audacity, as though they were waiting for an explosion of shocked disapproval. Surely their doctrine is something more than a tame consequence of the universally accepted view that linguistic rules are conventional. But what exactly is the novelty they wish to add?

I suspect that some philosophers who profess conventionalism may be under the impression that we can vary the rules of usage of our symbols at will *without altering the meaning* of those symbols. According to this interpretation

conventionalism would at least be something startling ; but a little reflection shows that it would also be quite untenable. For sounds and shapes acquire meaning only by being made subject to restrictive rules of usage, and an alteration of the rules must alter the meaning. If I decide to allow inference from " This is green " to " This is red " (*i.e.*, if I lay it down that " This is red " must not be denied when " This is green " is asserted), I adopt a new code in which one or other of the sounds " green " and " red " no longer has the same meaning as in English. If, on the other hand, I decide to use these sounds with the meanings that they have in English, I am not at liberty to change the rules which govern their usage. I cannot, for example, abandon the rule which forbids their use in conjunction. What is more, if there exist in any other language two words which fulfil the same rôles as " green " and " red " fulfil in English, those words must be subject to corresponding restrictions.

Since these points seem very obvious, when once they have been stated, it may perhaps be doubted whether any one has ever maintained the absurdity mentioned above. My reason for attributing it to some conventionalists is the fact that they talk of necessary *propositions* as true by linguistic convention. The proposition that $2 + 2 = 4$ is not itself a set of symbols, although we must use symbols from some language in order to refer to it. If, then, anyone supposes that the truth value of this proposition could be altered by an alteration of the rules for the use of the symbols " 2," " +," " = " and " 4," he must be assuming in a muddled way that a change of rules of usage which sufficed to alter the truth value of the *clause* " $2 + 2 = 4$ " might nevertheless leave it still expressing the proposition that $2 + 2 = 4$.

I derive no help, therefore, from Mr. Britton's references to rules. If conventionalism is the doctrine that we might have adopted other rules for the use of the sounds and marks we now use as symbols, it is platitudinous. If it is the doctrine that we could vary all the rules of usage of our symbols while leaving their meanings unchanged, it is

absurd. I hesitate to identify Mr. Britton's thesis with either doctrine, but I cannot find any third interpretation.

Mr. Urmson insists, quite rightly, that the possibility of having alternatives is implied by the doctrine that necessary truths are true by convention. His own view seems to be that some necessary truths have alternatives whereas others have not. Indeed, he thinks that, if the conventionalists confined themselves to saying that *some* necessary truths have alternatives, their contention would be obviously correct, because they could settle the matter by producing indisputable examples. He writes: "We have several alternative geometries, and commutative and non-commutative algebra, that is to say, there is an algebra in which " $a \times b = b \times a$ " is a necessary truth, and an algebra in which it is not." Now in a discussion like ours an agreed example is very valuable, and I agree with Mr. Urmson at least in thinking that his references to geometry and algebra are fair examples of the detailed assertions that conventionalists may be expected to make. It appears to me that a great deal of modern talk about the dependence of necessary truths on convention has been inspired by reflection on the developments of mathematics which he mentions. It is important, therefore, to consider these examples carefully. Here, if anywhere, it seems, we may expect to discover the correct interpretation of the conventionalist thesis.

When Mr. Urmson speaks of alternative geometries, I do not think he is referring to the distinctions between topology, projective geometry and metrical geometry. For these studies are alternatives only in the sense in which physics and chemistry are alternatives, *i.e.*, a person may study one without studying the other; and perhaps even this analogy is not quite appropriate, since the topological properties of figures *are* presupposed by their projective properties, and these in turn by their metrical properties. We certainly do not have to *choose* between topology and metrical geometry, holding to the one and rejecting the other, any more than we have to choose between the calculus of unanalysed propositions and the calculus of propositional

functions in mathematical logic. I assume, therefore, that the several alternative geometries mentioned by Mr. Urmson are the geometries which have been distinguished in the course of discussions about Euclid's parallels axiom. This is obviously the most hopeful interpretation of his example.

For convenience of reference let us first introduce short names for certain propositions according to the following scheme :

"E" for Euclid's axiom of parallels, or rather for Playfair's substitute, *i.e.*, the proposition that through a given point there can be drawn one and only one line parallel to a given straight line ;

"R" for the conjunction of the other axioms of Euclidean geometry as they may be found in some rigorous modern presentation, *e.g.*, in Hilbert's *Foundations of Geometry* ;

"M" for the proposition that many lines can be drawn through a given point parallel to a given straight line ;

"N" for the proposition that no lines can be drawn through a given point parallel to a given straight line.

At one time it was thought that E and R were both necessary truths. Non-Euclidean geometry started with the attempts of Saccheri and others to improve Euclid's exposition by showing that E followed from R and so need not be taken as an axiom. It was thought that this could be established by an *ad absurdum* proof in which the conjunction of R with the negation of E was found to entail a self-contradiction. But the effort to construct such a proof failed ; for no self-contradiction was derived from the conjunction of R with the negation of E. On the contrary, it was presently realised that E has two alternatives, namely M and N, either of which can be conjoined with R to produce a self-consistent system. The admission of M and N as real alternatives to E implies, however, the abandonment of the view that E is a necessary truth ; and so the word "axiom" is now to be understood in all geometry, whether Euclidean or non-Euclidean, as equivalent to "postulate." Where, then, are the alternatives to necessary truth which Mr. Urmson suggests we may find in non-Euclidean geometry ?

Surely not in a statement such as "If R and N are satisfied, the interior angles of a triangle are together greater than two right angles." For this is in no way inconsistent with the corresponding statement of Euclidean geometry, "If R and E are satisfied, the interior angles of a triangle are together equal to two right angles." We do not have to choose between these two hypothetical statements. On the contrary, we can and do assert both. In short, where there are real alternatives, *i.e.*, in the composition of axiom sets, there is no necessity, and where there is real necessity, *i.e.*, in the entailment of theorems by axioms, there are no alternatives. The moral to be drawn by philosophers from the development of non-Euclidean geometry is nothing like conventionalism, but simply the cautionary remark that propositions which are not self-evidently necessary may sometimes appear so because we have not tried hard enough to conceive alternatives.

Let us now consider the alternatives which are said to be found in the development of algebras. Mr. Urmson states that in one algebra " $a \times b = b \times a$ " is a necessary truth, in another not. Clearly he cannot be thinking here of abstract algebras, *i.e.*, uninterpreted calculi. For the expression " $a \times b = b \times a$," considered as part of an abstract algebra, is not a necessary truth, or indeed a truth of any kind, but merely an uninterpreted formula. He must therefore have in mind the various ways in which the symbol " \times " is used in connexion with numbers of different types. In order to understand this we must make clear to ourselves what we have done with this symbol in the course of the development of mathematics.

Historically, we all begin by using the sign " \times " for an operation performed on natural numbers. Then we go on to use it with signed integers, rational numbers, real numbers and complex numbers as its operands. For a rigorous development of mathematics all algebraic laws like the commutative law of multiplication should be proved afresh at each stage. The expression " $a \times b = b \times a$ " takes on a new sense when its variables have a new range of values ; and we have no right to assume without more ado that,

because it holds at some lower stage, it will hold also at the next higher stage. There are, indeed, a number of different universal propositions expressed together with systematic ambiguity by this formula as it is used in a non-rigorous development of mathematics. Multiplication of complex numbers is *not the same operation* as the multiplication of numbers of any lower type, although the propositions which hold true of it can be expressed by sentences of the same pattern as are used for expressing truths about multiplication at lower levels. The history of mathematics shows other examples of the interest of mathematicians in retaining old patterns of expression when dealing with new entities.

If we take all this for granted without a proper appreciation of our own procedure, we may be surprised when presently we find ourselves led on to talk of hyper-complex numbers for which the commutative law of multiplication does not hold. But we have no good reason for amazement. At this level multiplication has to be defined afresh, and there is no inconsistency whatsoever between our old statement " $a \times b = b \times a$," where " a " and " b " mark gaps for the symbols of numbers of any type up to and including complex numbers, and our new statement " $a \times b - b \times a$," where " a " and " b " mark gaps for the symbols of hyper-complex numbers (*e.g.*, quaternions). Since the mark " \times " does not mean the same in the two different contexts, there is no sense in talking as though we had discovered necessary truths which were alternatives to each other. What we have found are only alternative uses of the same mark. If we used a different mark to symbolize the operation which can be performed on hyper-complex numbers and ceased to call these entities numbers at all—as well we might, the whole mystery would disappear. Such puzzle as there is we have made for ourselves by using old symbols in a new situation with no more justification than a partial analogy.

I can find nothing then in these mathematical examples which helps me to understand the conventionalist thesis in philosophy. What exactly is the moral Mr. Urmson wishes to draw from them?

Both Mr. Britton and Mr. Urmson cite in support of their view a recent paper by Dr. Waismann on the question "Are there Alternative Logics?"* I have read this paper several times with the pleasure which I get from reading anything written by Dr. Waismann, but I confess I do not understand why he should link his views with conventionalism, as he does in his first paragraph. He has indeed shown that there are alternative logics, but only in a very wide sense of the word "alternative" which makes it equivalent to "distinguishable" and I doubt whether this is enough to satisfy those who call themselves conventionalists. In order to make my point as clearly as I could wish it would be necessary to consider each of his examples in turn, but this would take much too long, and I shall therefore notice only two which seem to me especially interesting.

The first example I wish to consider is the system of intuitionist logic first sketched by Brouwer† and then later formalized by Heyting‡. This is undoubtedly different from the system of classical logic, *i.e.*, the system expounded with varying degrees of generality by Aristotle, Boole, Frege, Russell and Lewis, and yet it can properly be described as logic, since it provides patterns of valid inference. But what exactly is its difference from classical logic? All the exponents, following Brouwer, say that its peculiarity is the dropping of the law of excluded middle. This seems to me a very curious way of putting the matter. It is true that the new logic can be obtained from classical logic by an omission, but it is surely a mistake to suggest that the new logic contains a sign with the ordinary meaning of "not" and yet differs from classical logic in failing to prescribe for this sign one of the rules which give "not" its ordinary meaning. It is more correct, I think, to say that intuitionist logic contains no symbol equivalent to "not" and therefore cannot contain the law of excluded middle

* *Proceedings of the Aristotelian Society*, vol. XLVI (1945-6), p. 77.

† *E.g.*, *Intuitionistische Mengenlehre* in *Jb. D.M.V.* 28 (1920), and *Über die Bedeutung des Satzes vom ausgeschlossenen Dritten* in *Journ. für Math.* 154 (1924).

‡ *Die Formalen Regeln des Intuitionistischen Logik* in *Sitzungsb. des Preuss. Ak.* (1930).

or any other logical principle concerned with ordinary negation. This interpretation is suggested by the phraseology of Brouwer in some of the papers where he gave the first informal exposition of the system. Instead of talking of negation, as one might expect if the question at issue were about the traditional law of excluded middle, he speaks of absurdity. He says, for example, that we can always argue from the truth of a proposition to the absurdity of its absurdity but not *vice versa*.^{*} We have here a clue which enables us to understand how the new system is related to the classical.

Let us suppose that we have a version of the propositional calculus of ordinary modal logic with “&,” “ \sim ” and “ \Diamond ” as primitive symbols, standing respectively for conjunction, negation and possibility. It is well known that with these all the ordinary concepts of the propositional calculus can be defined and all the ordinary principles formulated. Let us suppose in particular that the symbols “ \vee ,” “ \supset ” and “ \neg ” are introduced according to the following rules of abbreviation :

“ $p \vee q$ ” for “ $\sim (\sim p \& \sim q)$,”

“ $p \supset q$ ” for “ $\sim (p \& \sim q)$,”

“ $\neg p$ ” for “ $\sim \Diamond p$,”

and that all principles which can be formulated by the use of these abbreviating signs are so formulated. If we now strike out from the system all formulæ which contain the symbol “ \sim ” or the symbol “ \Diamond ,” what remain are the formulæ of Heyting’s system, although they must naturally be re-arranged for presentation in a calculus. In this lesser system the four symbols “&,” “ \vee ,” “ \supset ” and “ \neg ” are independent, *i.e.*, none of them can be defined in terms of the others, but all their mutual relations remain unaltered, and so they may be said to retain the same sense as in the fuller system. There are indeed some logical principles concerning these four logical notions which cannot be asserted in the lesser system, but this means only that a

^{*} *Intuitionistische Zerlegung mathematischer Grundbegriffe in Jb. D.M.V.* 33 (1925).

symbol which is required for their formulation cannot be provided from within the resources of the system.

While we can restore " \Diamond " as an abbreviation for " $\neg\neg$," we cannot define " \sim " in terms of the four primitives of Heyting's system, that is to say, we cannot so define it that it has the same sense as in the original system. It is therefore impossible to state the classical law of excluded middle :

$$p \vee \sim p,$$

or indeed any principle which involves ordinary negation, *e.g.*, the law of non-contradiction :

$$\sim(p \& \sim p).$$

Exponents of the system usually speak of " $p \vee \neg p$ " as the formulation of the law of excluded middle, and say it is characteristic of the system that it neither contains this formula nor admits of its insertion. They are correct, of course, in saying that the formula is alien to the system, but I wish to maintain that they are mistaken in supposing it is a formulation of the law of excluded middle, as that is ordinarily understood. According to the interpretation which is suggested by Brouwer's phraseology and confirmed by the derivation I have just outlined, the formula " $p \vee \neg p$ " must mean the same as " p or it is impossible that p ," and no one, except perhaps some absolute idealists, has ever wished to assert that as a general principle of logic.

It seems, then, that intuitionist logic is a fragment of classical logic rather than an alternative in the strict sense of something incompatible with classical logic. But why, it may be asked, should the intuitionist construct this narrow calculus for himself? Is it not because he wishes to reject all arguments that cannot be presented within his calculus? I think not. Brouwer has stated repeatedly that he has no objection to the law of excluded middle when its application is limited to propositions about finite sets. This is as much as to say that he does not entirely reject that part of classical logic which is excluded from the intuitionist calculus. His thesis is concerned only with propositions

about infinite sets, and what he says is that we should not try to establish such propositions by assuming the truth of the formula " p or it is impossible that p ." He thinks we shall be safe from falling into this error if we use in mathematics only arguments which can be formulated within Heyting's calculus. Now, although the formula I have just quoted may never have been asserted as a general formula of logic, it has been assumed to hold for propositions of the kind in which Brouwer is interested, namely, those of pure mathematics. Anyone who claims that every mathematical problem is soluble asserts in effect that, if p is any proposition concerned only with notions of logic and pure mathematics, then either p is true or p is impossible. And so Brouwer's thesis comes to this: when trying to solve a mathematical problem we are not entitled to use any argument which rests on the assumption that every problem is soluble.* Whether or not he is right in seeking to impose this restriction on mathematical proofs seems to me at least a sensible question, and I do not see how it helps to say that we can settle the matter as we choose by arbitrary convention.

The second of Dr. Waismann's examples which I wish to consider is that of many-valued logic. In recent years a number of logicians have tried to work out a many-valued system of logic, but none has won general acceptance for his system, because so far none has been able to interpret his calculus in a way which justifies him in calling it a new logic. Dr. Waismann suggests, however, that a many-valued system which deserves the name of logic can be constructed with the notion of graduated negation. It may perhaps be objected that the illustrations by which he tries to show the need for such a system are all of a rather limited kind and so do not entitle us to talk of the proposed calculus as logic. For there are only a few special fields, such as the estimation of measurements, in which we can talk reasonably of infinitely many degrees of falsity, and

* Cf. his *Intuitionistische Betrachtung über den Formalismus* in *Sitzb. des Preuss. Ak.* (1928).

logic is not concerned with what is peculiar to special kinds of subject matter. I shall not try to express an opinion on this question, but content myself with pointing out that, if a many-valued logic can be developed on the basis of Dr. Waismann's interesting suggestion, it will be related to classical logic much as classical logic is related to intuitionist logic, that is to say, it will contain classical logic as a proper part and provide for the definition of a concept which is taken as fundamental in classical logic.

For simplicity let us consider first a three-valued system in which " \sim ," the symbol of ordinary negation, does not occur but there are instead two primitive symbols, say " $*$ " and " $!$," which divide the work of negation between them. In ordinary English these two symbols may perhaps be translated by the phrases "not quite" and "not at all," but such translation is misleading if it suggests that the symbols have sense only through a definition which contains the word "not" or some equivalent; within the system they are to be taken as undefined. If, however, there exists within the system a symbol of disjunction (*i.e.*, a symbol with rules *analogous* to those of disjunction within the ordinary two-valued system), ordinary negation can be re-introduced by definition as a disjunction of the two operations which have been said to divide its work. We then have " $\sim p$ " in our system as an abbreviation for " $*p \vee !p$." In a system with more than three truth values the definition of simple negation would naturally be more complicated, but the mere number of distinguishable truth values would not introduce any new difficulty of principle. On the contrary it should make clearer what is happening. In this connexion Dr. Waismann himself writes: "One might here apprehend ordinary negation as an equivalent of a sum of operations in a logic with infinitely many truth values. In other words, ordinary negation can be split up into an infinite disjunction of precise operations; I say precise because each of these operations leaves the communication value of the original statement unaltered. By extending the disjunction over an increasing number of terms we 'dilute' the original statement more and more;

the limit of this process of diluting is ordinary negation. Thus certain features of ordinary logic are fully understood only in the light of many-valued logic.”* But this is surely as much as to say that, when we interpret a many-valued calculus in a way which justifies us in calling it logic, the calculus turns out to be an amplification and refinement of classical logic rather than an alternative in any strict sense. And if this is so, I do not see why we should suppose that talk of conventions will help to illuminate the situation.

(5)

Although I cannot understand the thesis of the conventionalist in detail, I think I can guess the reason why they try to explain necessary truth as a product of convention. They believe that in this way, and in this way only, they can escape from the dangers of talking about intellectual intuition. Now it must be admitted that philosophers who appeal to intuition often report different answers from their oracle. Some indeed report nonsense. It may therefore seem hard-headed to say that all necessity is *imposed* by our own legislation. But if this is the conventionalist's motive for developing his theory, he is doomed to disappointment. Whenever he hopes to get clarity and determinateness by setting out his rules and considering their implications, he will find that he is forced to rely once more on intellectual intuition, although this time it may be only insight into logical connexions. The effort to dispense with intellectual intuition is, indeed, an attempt to dispense with all thinking; for anyone who notices that something is of a certain kind, *e.g.*, red, must be intuitively aware of the mutual exclusion of some kinds, *e.g.*, red and green. But the absurdity of the enterprise is especially clear in the limiting case just mentioned, where it is supposed that the conventionalist has only to apply rules of his own making.

Perhaps Mr. Britton has this objection in mind when he insists that agreement is the sole test of correctness in the

* In the article cited, pp. 90-1.

application of rules. His emphasis on this statement suggests that he is trying to be as thoroughly conventionalist as he can at a point where he anticipates attack. Now everyone will allow that if two attempts to apply a rule, whether made by different men or by one man at different times, yield different results, one at least must be faulty, whereas if two attempts yield the same result, there is some presumption that they are both correct. And it may even be said that agreement with other attempts is the sole *external* test of the correctness of any attempt to apply a rule. But it is surely conceivable that all the attempts to apply a given rule which are made within some finite period are incorrect together ; and so the correctness of attempts to apply a rule cannot be *defined* as agreement between the results of the attempts. Yet nothing less would enable Mr. Britton to evade the admission of a necessity which is not imposed by convention. For so long as we distinguish rules from descriptive generalizations about human behaviour and talk in the ordinary way of the correctness or incorrectness of attempts to apply them, we assume in effect that there are principles of logic independent of our conventions.

DOES PSYCHOLOGY STUDY MENTAL ACTS OR DISPOSITIONS ?

MR. W. B. GALLIE, MR. W. J. H. SPROTT AND
PROF. C. A. MACE.

I.—*By* W. B. GALLIE.

Introductory.—Far too often, in discussing this or kindred questions, philosophers slip into asking “What can or may or ought psychology to study ?” and give answers, according to their different philosophical predilections, as to what it would be useful (or at least harmless) or right (or at least not senseless) for psychology to study. My aim is, as far as possible, to decide what psychology does study, through an examination of its actual procedures. To attempt this is not simply to compliment psychology on having “entered upon the secure path of a science,” it is, I believe, the only way philosophers can help psychologists to maintain their advance on that path. My own knowledge of much of the relevant material is sketchy and secondhand, and the answers I shall give may be far too sweeping ; on either score, the psychologists who follow me can make the necessary corrections. But in defence of what follows there are two remarks I should like to make : first, my questions and answers are put forward as a kind of challenge to psychologists, as an attempt to elicit from them a clearer statement of some of their aims and methods, and to make them recognize the need of some major rectifications in their terminology. Secondly, I feel the best way of getting my challenge across is to state my own conclusions in sweeping, indeed, perhaps paradoxical, fashion ; for if these conclusions be wrong they will at least be strikingly wrong, and in showing why they are so the psychologists who follow me will be doing something for their science which, I am convinced, badly needs doing. My main conclusion is indeed simple and sweeping enough. It is that, in so far as psychology studies what can be properly

described as *acts*, it never conceives these as *mental*, and that, conversely, in so far as psychology studies things mental, or minds, it never does this through an investigation or analysis of mental acts. My reasons for maintaining this can best be shown by breaking down our original question as follows.

(1) What, if anything, is there in common between the various uses of the word act as found in recent psychology? In the light of my answer to this I shall ask, emphasizing the word *acts*; (2) does the description of certain mental phenomena (or of certain mental "inferred entities") as *acts* serve to bring out features of those phenomena (or inferred entities) that are psychologically important, *i.e.*, such that psychology does try to analyze and explain them? In answering this I shall *ipso facto* be giving my opinion as to the width of the class of entities that are, in a psychologically important sense, acts. In the light of this, I shall ask, emphasizing the word *mental*, (3) when psychologists properly describe certain objects of their study as acts, are they concerned to show that such acts are *mental* or are acts of *minds*, and, further, is the presupposition that such acts are mental necessary to the character of their investigations? Now my answer to both parts of this question will be No. It is therefore pertinent to ask: (4) if such acts as psychology studies are not studied *qua* mental, how do they come to be of interest to psychology? and (5) if minds are not studied by psychologists in terms of, or through a study of, mental acts, how *can* minds be studied by psychology—as they quite certainly *are*?

What, if anything, is common to the various uses of the word act as found in recent psychology? That the word act has been used in psychology in fundamentally different senses could, I think, be proved in a number of ways; but all I can attempt here is to sketch one line of proof based on a consideration of the very different derivations of different uses of the word. (a) Some of these are taken over direct from everyday speech, *e.g.*, the phrases act of choice,

deliberate act, act of skill, habitual act, instinctive act, along with their synonyms and cognate verbal forms. These different phrases suggest commonsense distinctions between what is attended to and what is not, what has been learnt and what has not, and so on. Let us assume that the word act stands for the same kind of entity in all these cases*: then we may contrast these uses of the word with others; (b) whose derivation is obviously quite different, *e.g.*, the phrases reflex act, iodeo-motor act, integrative action, all of which stand for species of the genus *behaviour* or *organic response*. Uses of this latter sort point to problems (*e.g.*, as to the physical determinants of sensation or the mutual dependence or independence of different kinds of response) which call for entirely different methods of investigation and, as it seems to me, receive entirely different kinds of answer from those given to *most* questions regarding what is learnt and what is not, what is attended to and what is not, and, *a fortiori*, what is personally and/or morally attributable and what is not. This point can perhaps be seen most clearly in those discussions of psychological procedure in which Behaviourists have argued that acts or movements or responses are the proper units of psychological study; their reason being that only such acts or movements can be publicly observed, isolated from disturbing factors, accurately measured, etc. Now, whether the same can or cannot be said of acts of choice, skill, habitual acts, etc., is not relevant to my present argument: what is relevant is that acts of this latter sort have obviously *not* been chosen for psychological study for the same reasons Behaviourists urge for the acts *they* wish to study. Different psychological schools, it thus appears, have been advancing different lines of psychological interest, even though their act or action terminology may have disguised this.

(c) I wish next to note a use of the word act that has been introduced by certain philosophers of psychology, Dewey and Mead in particular, and which has had a considerable

* For reasons that are suggested below I, in fact, doubt this, since some of the acts listed admit of moral and/or legal predicates while others do not.

influence on some psychological schools. Mead's "philosophy of the act" is, in its fully generalised form, a metaphysical hypothesis ; but in its more limited (and plausible) form it is an attempt to state the common structure of all those behaviour patterns within which intelligent behaviour arises. Now it might be thought that such a conception presupposes something common to all the different kinds of act studied in psychology. But in view of Mead's strong sympathy with the behaviourist programme it is almost certain that he would have wished to *eliminate* certain uses of the word act from psychology ; and what can be eliminated from a given system of ideas cannot have anything in common with the rest of that system—unless it be a sheer redundancy : it is much more likely to be an "alien body" ascribed to the system through some verbal confusion. There is therefore, I think, no presumption that Mead's use of the word act—as I shall urge below, a most important one—implies something common to all other uses of it. (d) Lastly I wish to note a sense of the word act that is derived from the theory of knowledge, as in, *e.g.*, the phrases act of judgement, act of knowing, act of supposal, consideration, etc. Here I wish only to stress that, for the purposes of theory of knowledge, what is meant by an act of this class is simply whatever mental phenomenon (or inferred entity) corresponds to one of a given proposition's powers of determining the truth or falsity of certain other propositions. This being the sole interest for theory of knowledge of acts of this class, it seems safe to assume that, if or in so far as they are studied by psychology, they must be studied by quite different methods from those that apply to acts of the other classes I have discussed.

This brief survey will serve, I hope, to support the claim that the word act as used in psychology is ambiguous and liable to mislead not only laymen reading psychology, but psychologists themselves in describing some of their objects of study.

Does the description of certain mental phenomena (or inferred entities) as acts serve to bring out features of them that are psychologically important ? The psychological interests and pro-

blems in connection with which the word act is most usefully employed make up at first sight a somewhat heterogeneous collection. All the studies of "reflexologists" fall into it; so do many of the studies of educational industrial psychologists, *i.e.*, those concerned with acts of skill, habitual action and the like. Let us first ask why reflexes can usefully be described as acts. The answer seems to be (*a*) because a reflex is a more or less isolable phenomenon, (*b*) because every phase of it can be linked back causally to a first phase, its stimulus, and (*c*) because its last phase, the overt response, has a fairly fixed spatio-temporal relation to its first phase, the stimulus. This seems to be the justification of using the word act not only of reflexes but of a number of more complex responses. Somewhat similar considerations apply also to skills. These can be analysed into parts which (*a*) can be attended to separately, (*b*) can be separately described or demonstrated by a teacher, and (*c*), when learnt, will stand to parts of the pupil's activity (their "cues") in much the same way that, while they were being learnt, they stood to or followed on the teacher's instruction. Such parts can, then, be usefully considered as single acts or movements, to be prescribed and learnt.

With regard to habitual acts the matter is somewhat more complex. A psychologist may want to find out whether a certain kind of act can become habitual, and if so how long on the average this will take, and in what circumstances the formed habit may be modified or lost. In studies of this kind the psychologist's presupposition is that he can trace back causally particular instances of the habit to the initial acquiring of it, and, further back, to the first occurrence of the action or movement that later becomes habitual; and thus far he studies particular acts, varying their conditions, etc., in ways that have much in common with the reflexologist's procedures. But a psychologist may also be interested in a particular person's power of taking or losing habits, relative to other persons; and when this is the psychologist's *main* interest then "act-analyses" of habit fall into the background. Instead, as in

intelligence and emotion tests, the psychologist gives us information in terms of performances, gradings, capacities (and always, I think, capacities of a more general order than the capacity for taking or losing the particular habit under study). Here we come upon a fundamental dichotomy in psychological methods, some of whose implications will concern us later. What I wish to stress here is that this second method (explanation in terms of general capacities) can be applied to problems of learning, attention, habit-taking, etc., quite independently of analyses of these acts in terms of their component parts or particular causes.

Let us now turn to those uses of the word act that are derived from the theory of knowledge. Obviously, many psychological statements (like many commonsense statements) relate to acts in this sense ; indeed it would be impossible to talk for long about minds or mental phenomena without referring to them. But, as I shall try to show, it does not follow from this that psychology *studies* such acts. For convenience, let us call knowledge, judgment, supposal, doubt, etc., specific forms of "propositional attitude" and acts of knowing, judging, etc., instances of these specific forms ; then we might say that every such act corresponds to *one* of a given proposition's powers of determining the truth or falsity of certain other propositions. Thus when I claim to know that I am thirty-four years old I am logically committed to rejecting any contradictory suggestion. When, on the other hand, I simply judge that I am thirty-four I am committed to considering any contrary suggestion provided it has some appreciable evidence to support it. Again, when I suppose that I am thirty-four I am committed *not* to assert any consequence of this proposition independently of my original supposal ; and so on. Now quite obviously the relations thus traced between these different acts correspond exactly to certain logical truths or truisms, *i.e.* that a proposition and its contradictory cannot both be true, that no degree of probability less than unity precludes the possibility of a proposition being false, that the only premisses that can be validly dropped from an inference are categorical ones. This suggests that an important part of

the meaning of acts of this class is that to know or judge or suppose is to express (to oneself or another) an acceptance of certain logical restrictions and compulsions within a given field of discourse. But equally clearly, it may be claimed, one is doing something more than this in knowing or judging or what not. To this I answer readily that I am quite sure one is ; for, every organic response being a highly complex affair, one is presumably doing a large number of things in knowing, judging, etc. Something much more specific than this, however, is meant when it is claimed that in knowing judging, etc., one is doing more than entering into certain logical commitments. This might be brought out by saying that always in such cases one is in a direct relation with something, the object of one's knowing or the occasion or subject of one's judgment. Acts of knowing, judging, etc., it might then be claimed, consist in getting oneself into such relations and persisting in them ; and the logical restrictions and compulsions I mentioned above would then be necessary consequences of such acts, following from their definitions or natures. We are therefore faced with the question : does psychology study acts in the sense just described ? Does it study acts which can be identified as acts of knowing, judging, etc., and study them in respect of their being such ?

I don't think it does. For one reason, I don't see that any useful purpose would be served by a psychological study of such acts. We all know already how to identify them in our own experience : thus I know that I know when, for example, I possess an answer which, on whatever grounds I consider conclusive to a given question ; I know that I doubt when I am busy in certain characteristic ways because I lack such an answer. Granted, we all on occasions use such words as know, judge, doubt, etc., slackly ; but this is because we are all in some measure logically slack and slovenly ; and certainly it isn't through becoming psychological students (or patients) that we learn to tighten up this part of our vocabulary. In the second place, when we examine psychological analyses which claim to be "of" acts of this kind, into what kinds of component do we find these acts broken down ? Always, I think, into components

that on careful examination can be seen to be neither severally nor in combination *acts* in the sense that theory of knowledge requires. The components of such analyses can be subsumed under three heads : (a) sensations, images, sentences spoken or imagined or semi-articulated ; (b) vague entities (*e.g.* Driesch's *Zeichen*) which sometimes precede expressions of knowledge, judgment, etc. ; (c) discriminable "logical moments" in perception or memory or in Professor Spearman's "neogenetic" acts.

With regard to (a) no argument is necessary : no one has ever claimed that sensations, images, etc., are acts. Components of the second kind (b) are altogether too vague, and shapeless and variable as between different introspectors to be identified with acts of the kind we are discussing. Assuming that such acts *were* (which I am going in a moment to deny) things or events that can be spatio-temporally isolated and located and whose causal connections can be traced, then it might seem plausible to consider such entities as Driesch's *Zeichen* as ingredients of them ; on the other hand there would seem to be just as good a case for considering such entities as accompaniments of these acts, in the way that images often are, the more so as it is the most careful experiments on this and kindred issues that show the greatest variety of results as between different observers. Components of kind (c), it might well be thought, can occur in psychological analyses only through some confusion ; and indeed too often in attempted descriptions of perceptual or memory "processes" different "logical moments" (or transitions from an act that is an instance of one propositional attitude to an act that is an instance of another) are recklessly identified with different introspectible, or causally inferrible, stages in the perceptual or memory process. Now even if such identifications were justifiable, they would do nothing to explain acts in the sense we are discussing ; since the discriminated stages would obviously be acts in just the sense that needs analysis or explanation. There is, however, one celebrated analysis of "knowing acts" which at first sight seems to be of this kind but which nevertheless is exempt from both the above criticisms. According to

Professor Spearman every act of knowing or "neogenesis" involves each of his three qualitative laws or principles, Apprehension of Experience, Eduction of Relations, Education of Correlates. Here again, it might be thought, we have simply the substitution of three unknowns for one. But in fact each of Spearman's three principles is easily identified by commonsense reflection: (indeed one can actually see a testee educating the right or wrong relation or correlate under test conditions). What Spearman's "analysis" of knowing acts provides is a method of deciding, in cases where introspection is impossible or untrustworthy, whether an act of knowing (or learning) has occurred, irrespective of its inner nature or "analysis." But for this very reason what Spearman gives us is not a psychological analysis of such acts at all. It is something much more commonsensical—and useful. Spearman shows his superiority (his superior sense of what scientific investigation is or should aim at) over all analytic psychologists in this field by in effect refusing to do what they attempt to do. His "Principles of Cognition" genuinely subserve his positive enquiries into the "Abilities of Man," in as much as his three qualitative principles offer us a method of deciding whether someone's ability to know has in fact been realised in a given situation. (I shall be concerned below with the fact that all Spearman's positive findings with regard to knowledge, learning, intelligence, etc., relate to capacities or abilities to learn or know, not to the character or analysis of "knowing acts.")

From this discussion I conclude that psychology does not study, in the sense of analysing, those acts that are instances of "propositional attitudes"; and I believe that, by a closely similar line of argument, the same conclusion could be reached regarding those acts, expressed by the words choose, decide, intend, promise, etc., which are instances of attitudes, not to propositions, but to possible lines of moral (morally attributable and answerable) action. It might, nevertheless, be claimed that psychology studies acts of both these classes in respect, not of their inner natures, but of their causes and effects: for example, are

not such acts explained, to some degree at least, by the laws of association or through a study of their physical determinants? The first suggestion can be ruled out at once: there is, I think, sufficient agreement to-day that it is not occurrent ideas or states or acts of mind that get to be associated, but capacities or tendencies or dispositions to have ideas or perform acts. (The question of how capacities, tendencies, dispositions are studied by psychology remains to be considered.) The second suggestion is in no better case: physiological psychologists have discovered many important things about the causes of sensation and about the causal relations of various sorts of bodily response; but of the physical causes of acts of the kind we are discussing they have told us next to nothing. I therefore reach the more general conclusion that as regards acts which are instances of either propositional attitudes or moral attitudes, psychology has nothing to tell us. And in case this should seem an example of the silly philosophic habit of trying to set bounds to the scope of a natural science, I will try briefly to show that I am doing nothing of the kind, in as much as the acts we have been discussing are not "natural phenomena" or entities inferred from such phenomena, and therefore are not part of the subject-matter of psychology or of any natural science.

When philosophers have written of these acts they have not meant by them certain isolable events, whose structures could be analyzed and antecedents and consequences causally determined. In other words, the uses of the word act we have been discussing do not imply that there are events which are acts of knowing, judging supposing, etc.; although of course it is perfectly correct for someone to say of a certain event that he has had direct knowledge of it, or has judged or supposed something with regard to it. But if it is asked what such knowing or judging consists in, we can, I think, answer in only two ways: first, as I have already said, my knowledge or judgment is shown in my disposition to do and to utter certain further things with regard to what I claim to know or judge, and not to do or utter (or listen to) certain other things. But I also claim

—and here we come to the second answer—to know when I know, to know when I judge, suppose and so on. What, then, does this claim amount to? Primarily, I think, that the kind of situation or context in which I claim to know or judge, etc., is familiar to me and that I can and do describe it correctly as the kind in which one knows or judges. (It is the kind of situation in which by saying the words “I know” or “I judge” I commit myself one way or another, for better or worse, to certain logical restrictions and compulsions in the future; or, more accurately, *in potentia*, irrespective of the actual course of my future experience.) But in thus recognizing what I am doing in claiming to know or judge, I am, so to say, warned (if I have a logical conscience) that I have made many other such claims in the past, which *ought* to be restricting and compelling me in certain ways *now*. Thus, in any instance of knowing, judging and the like, I am not only knowing what I do because I am doing the *kind* of thing I have done before (this is the reason why such “acts” are “self-conscious”), what I am doing now ought to proceed on certain logical lines laid down by previous instances of knowing or judging or the like (this being the reason why such “acts” are *personally* attributable and answerable).

If this account of the matter is correct, then it follows that no “act” of the kind we have been discussing would *be* an act of knowledge or judgment or what not if it were isolated from its logical debt or due to—or inheritance from—the past, or from what it commits one to logically, *in potentia*, in the future. This inheritance and commitment, it must be emphasized, have *per se* nothing to do with causality: so called acts of knowledge, judgment, etc., cannot be themselves causal agents or results because they are not, *qua* cases of knowledge, judgment, etc., isolable items or events in nature. Hence it would be safer, less misleading, never to speak of such “acts” at all, but only of instances of knowledge, judgment, supposal and the like—in the same kind of way that we speak of instances of qualities or laws. Thus I would say that the word “knowledge” is best defined as standing for (*a*) the sum of a man’s

experiences in so far as these are ordered in certain ways (that commit him in certain ways) in accordance with certain parts (those that relate to his use of "knowledge" words) of his *logica utens*, and (b) the applicability (as a result of whatever causes) of his past experiences as thus ordered to any of his future experiences. When a man's knowledge as thus defined is applied to some new experience, we can safely say that he gets or received new knowledge or adds to his knowledge. But we are then saying nothing about "what happens" in (or "behind") his experience in such cases; and we only fool ourselves if we employ an act-terminology that suggests we are describing or referring to "what happens." No doubt all manner of things are happening in such cases—many kinds of neural events some of whose properties may well be *causally* necessary to our actual effective *uses of the word* knowledge. But, quite obviously, such events or their properties are not themselves knowledge. Knowledge, judgment, belief, supposal and the rest get realized in the world of causation—of events, sensations, utterances, movements, and provide that world with such "reasonableness" as it possesses; but they are not themselves events—or sensations or utterances or movements. This is why some contemporary philosophers have described knowledge, judgment, belief, etc., as *fundamentally dispositional*. In saying this they have emphasized one aspect of the matter, the reference of all such words to the potential future. But they neglect or at least fail to emphasize the other side, the reference of such words to what I have called logical debt to or inheritance from previously achieved knowledge, belief, etc. More justice has been done to this side of the matter by philosophers who have favoured "anamnesis" theories of knowledge, and, in so far as it relates to psychology, by Wordsworth when he exclaimed

" Hard task, vain hope, to analyze the mind,
If each most obvious and particular thought,
Not in a mystical or idle sense,
But in the words of Reason deeply weighed,
Has no beginning."

When psychologists properly describe certain objects of their study as acts, do they presuppose that such acts are mental? In the light of the foregoing discussion this question can be answered quite briefly. So far as reflexologists and "functional" psychologists are concerned, the answer is a definite No. Psychologists study reflexes, and other responses that can be described as acts for the same kind of reasons that reflexes are, without reference to the minds (if any) of the organisms under study, and often expressly repudiate (on sound empirical grounds) the relevance of minds or anything mental to acts of these kinds. Can the same be said of acts of skill as analyzed by practical psychologists? I am strongly inclined to think it can. Of course the psychologist who analyzes the characteristics of a given act of skill knows that the persons who can be taught to perform it have minds, and that if they hadn't they couldn't make use of his analysis. But in making his analysis the practical psychologist does not necessarily consider how his pupils' minds can be brought to bear on the task (unless his problem be concerned with directing and maintaining attention, which is a quite special case): normally the practical psychologist assumes his pupil's capacity to learn and can therefore treat the skill in question as a complex pattern of movements. The ambiguity of the word skill may sometimes obscure this, for by a skill may be meant either the way-the-job-is-to-be-done or someone's capacity to do it. But one very simple example will make my point plain: a shot in cricket or golf, can be analyzed and taught in terms of a number of factors, position and movement of hands, feet, weight, etc.; and a good coach is one who knows the analysis of *shots*, not one who has the capacity to instil a passion for cricket or golf. I conclude therefore that acts of skill are studied by practical psychologists without explicit or *psychologically important* reference to the minds of those who are to perform them.

The same answer must, I think, be given with regard to instinctive acts or actions, as studied in animal psychology. While most psychologists seem agreed that actions such as securing food and shelter, mating and care of young, etc.,

cannot be reduced to chains of reflexes, it does not seem to me that the hypothesis of a mind guiding or informing such actions is of any real assistance to the psychologist who studies them. (The hypothesis of a temporary set or disposition of the nervous system would appear to be a much more adequate and potentially testable one.)

I am thus committed to saying that, in so far as they study phenomena that can be properly described as acts—or that the acts in a psychologically important sense of the word—psychologists are not interested to show that these are mental, and that often, so far from presupposing a mind that guides such acts, psychologists expressly repudiate this idea. Combining this result with that of our previous discussion, I reach the conclusion that psychology does *not* study mental acts.

If the acts psychology studies are not studied qua mental or as acts of mind, why are they of importance to psychology? It would be easy to dismiss this question with the answer that psychology is not simply the science of minds, but is also the science of behaviour. But then two questions would face us: (a) are these two fields of psychological study entirely separate? and (b) how is behaviour to be defined? (a) Judged by the practice of psychologists, the answer to this question would seem to be that the two fields *can* be kept completely distinct. (For example, what has Spearman's account of the abilities of man in common with Pavlov's experiments on dogs?) Nevertheless such a radical division is unsatisfactory, and it can, I think, be overcome if we adopt a more careful definition of behaviour than has usually been given. (b) It is here that Mead's conception of "the act" can be of great service to psychology. For Mead, as I have said, "the act" means the characteristic structure of those organic responses within which "intelligent behaviour" arises. What the latter phrase means can be shown in three kinds of case, those in which physical objects are dealt with intelligently, those in which intelligent co-operation between organisms is achieved, and those in which an organism directs, in the sense of being able to correct or criticise, its own behaviour. To consider *only*

the first case, Mead sees the "genesis" (or "matrix") of the concept *physical object* in those behaviour patterns that contain a sequence of (i) distance signs (visual, auditory or olfactory sensations) (ii) a manipulatory phase (contact sensations through hands, paws or jaws at the locus of a distance sign), and (iii) a satisfaction phase (appeasement of some organic need). The unity of such acts lies in the tension set up through receiving a distance sign that stimulates a particular organic need ; but the act involves an *object* only when the total response is delayed—after either of its first two phases. Thus a bone to a starving dog is not an object, it is simply there to be eaten ; to a replete dog, on the other hand, it can be an object in as much as eating can be delayed—the bone sighted today can be left or buried, and eaten tomorrow. Alternatively we could say that in this latter case the dog acts as if it has recognised certain causal properties in the bone, it doesn't simply act *on* it, or on the sight of it. In Mead's picturesque language the object, in so far as it "exists" in and through such a delay in response, is "a collapsed act."

Now I think that behaviour could usefully be defined as a class of those organic movements that are parts of, or, more generally, that can help us to identify and follow, acts in Mead's sense. Thus the study of reflexes may well help us to identify and follow the mechanisms of some of the simplest acts in this sense. On the other hand, certain bodily changes, *e.g.* chemical changes resulting from food or sleep, would not on this definition count as behaviour. And I think everyone will agree that the study of such changes has no place in psychology. If, now, this definition of behaviour were adopted, the division between psychology as the study of behaviour and psychology as the study of minds would be at least less sharp and extreme. For behaviour would now mean parts or phases of those "acts" that provide the genesis or matrix of intelligence. Nothing is hereby implied about *reducing* intelligence to certain behaviour patterns, or about reducing minds to capacities for intelligent behaviour. Nevertheless a certain unification of the subject matter of psychology would be achieved.

If minds are not studied by psychologists in terms of mental acts, how can they be studied? An adequate answer to this question would involve—what, aside from the exigencies of space, I am quite incapable of giving—a thorough analysis of the characteristic aims and methods of contemporary psychology. Here all I can do is to point to the obvious facts that psychology does study minds, and that it has already discovered a great many important things about them; and to answer the charge that a study of minds can only proceed through, or in terms of, a study of mental acts.

It seems quite clear to me that, when all criticisms have been made, Spearman and the London school of psychologists have discovered a number of “new truths” about human abilities (in particular the ability to learn or know), truths that are applicable—and verifiable—in a number of practical ways; and that Freud and the psychologists who have learnt from him (including those who have learnt most through criticising him) have discovered, or at the least suggested, a number of things of the first importance about the “workings” of human minds. Again, for all that they scrupulously avoid the use of “mental” words, psychologists of the Gestalt school have, I think, suggested—through their conceptions of tension systems, equilibria, tasks, etc.—a really new way of regarding human and animal minds, though with what really important results to date I am not in a position to judge. And these contributions to the study of minds, it seems to me, have been made without the aid of analyses, either constitutive or causal, of individual “mental acts.”

How is this possible? For, it might be argued, even if minds have capacities to know and perform in certain ways, and tend to “work” along certain non-introspectible lines, these capacities and tendencies can only be studied through individual performances and workings which, even if they be not acts in the senses proper to theory of knowledge and ethics, are nevertheless entitled to the description “mental acts.” More specifically, have not Spearman’s most important conclusions been derived from the study of test-performances made under experimental conditions? Have

not the most characteristic Freudian doctrines been derived from a minute study of particular data—dream incidents and their connections and motives, utterances and their implications, and even parts of the structure of individual utterances (slips, confusions, hesitations and the like)? I will deal first with these two more specific queries, and then attempt a more general answer.

(a) What kind of knowledge does a psychologist hope to get from administering an intelligence test to a particular subject or testee? Primarily, he wants to know the subject's *general capacity to learn*, and he hopes to determine this through discovering the *level of performance* of which the subject is capable. Because of this the test is designed to ensure, as far as possible, that the subject's result or score shall be representative of his general capacity to perform or "score." The test is not designed—primarily at any rate—to display the subject's learning-capacity *in action* (for all that useful subsidiary results may be gained from seeing where he slips, fails to relate, remember, adjust, etc.). The interest of the test therefore is not in what it shows the subject doing (his "mental acts" or actions) but in what it tells us about his potential or capacity; and this relates not only to his future (what jobs or studies he will prove capable of) but to his past; for the test reveals at least the very general fact that the subject's previous education and activity have been such as to bring (or keep) him up (or down) to the level of performance as shown by his score. And this is my main answer to the first query. But let us return to the fact that useful subsidiary information can be gained from seeing where, in a test, a subject slips up, or, as may be, successfully adapts answers that are appropriate to certain earlier parts of the test to certain later phases of it. To take the latter case, it may be claimed that here the subject's acts—of learning, correlating, adapting—are being studied *per se* during the test. Two replies may be made to this claim: (i) Just as the test itself is a highly complex performance taking up several minutes, so any observable phase of it is a performance that cannot safely be identified with one or any numerable group or

set of "cognitive acts" of the kind analytic psychology attempts to study. (We may see a testee educating a right or a wrong relation but we can never say precisely how he did it, or even what "thoughts" occurred to him while doing it.) (ii) Even if we can sometimes see the subject's learning capacity *in action* during the test (as when he adapts an earlier answer to a later question) this itself is of interest only in so far as it is taken as representative of the subject's general capacity to make intelligent adjustments within a given task. If, by chance, the subject were to perform during the test a feat of intelligent adjustment for the first time in his life, he would, in a sense, have cheated the tester. For he would have manifested a very shaky or ill-established capacity, which the tester would perhaps misinterpret as a well established or reliable one. (In fact, of course, such a freak performance would be detected, from other parts of the subject's result; but this does not affect the argument that *particular acts* of learning are not of interest to the psychologist.) And this, I think, disposes of the first query.

(b) From some of Freud's statements about the need to account for the specific qualities or structures of certain dreams, statements, slips and other "symptoms," it might very naturally be inferred that psycho-analysis and the psychological theories based on it require a minute analysis of mental acts of certain kinds, and in particular a detailed tracing of their causal ancestry. But it requires only a little logical reflection to see that this inference is untenable. (i) A particular symptom *is* a symptom in virtue of its being one representative *among others* (actual or possible, more or less obvious, more or less violent or disastrous) of a chronic or periodic ailment, or of the inferred causes of that ailment. Alternatively, we may say that no symptom is ever interpreted in respect of its intrinsic properties or structure, on the assumption that these directly reveal the workings of a background cause or ailment (repression, conflict, complex or what not), but that a symptom is always interpreted in respect of certain relations—usually of a somewhat complicated kind—it bears to *other* symptoms (possible or inferred

or actually observed), and it is the whole system of symptoms taken together that suggests the workings of the background cause or ailment. This shows that the structure and causation of *particular* symptoms are of no interest to the psycho-analyst. (ii) If anyone thinks that Freudian or any other methods of analysis enable a psychologist to determine the specific causes of the specific qualities or structure of a given symptom, and *a fortiori* to trace out its causal ancestry in anything like completeness, he must, I submit, be very simple minded. And yet it seems to me that some people have believed this—have, as I suspect, confused this with the fact that every symptom is essentially one of a class of symptoms inferred or supposed to be “of” a certain background condition or ailment. Now certainly, if some of these other symptoms date from very early experiences of the patient, then these are commonly considered, on reasonable if debatable empirical grounds, to be the most important *from the point of view of effecting a cure*. This, however, suggests nothing about the possibility, or desirability, of tracing out the complete causal ancestry of the particular symptom either through direct reports or by causal inference. I submit again, therefore, that this discussion shows that psychologists can and do study minds (even their obscure remoter depths) without attempting either a constitutive or causal analysis of particular mental acts.

Conclusions.—(a) If, as I have tried to show, examination of psychological practice suggests that mental acts are not a part of the subject matter of psychology, then why should the belief that they are have arisen, particularly among psychologists themselves? I can suggest only the merest outlines of an answer to this. The belief in question seems to me to be a faulty offshoot from the valid belief that psychology should study mental action, working, performance, etc. (This is a sound reaction to eighteenth century associationism.) But there has followed on this a desire to “pin-point” such mental action, due either to an innate tendency of the human mind to look for more and more exact determinations of natural phenomena *only* through

pursuing them more and more minutely (by locating them more and more narrowly in space and time), or to a tendency, similar in effect, but attributable to the example of one particular branch of human enquiry, viz., physics. Whatever the origin of the tendency, I am committed to saying that, as far as psychology is concerned, it is misleading and wants correction ; and I think that many psychologists will agree with me in this.

(b) If, as I have suggested, psychology, in so far as it studies minds, takes certain dispositions, "sets," chronic or periodic conditions and the like as its "units of study," does not this commit us to viewing minds, if not as series of separate mental acts, at least as bundles of dispositions, sets, tendencies, etc. ; and is this way of viewing minds a plausible one? To this I answer that no general view (metaphysics) of the mind is implied in my suggestion, which relates simply to the ways in which certain things have been learnt, and certain further things will be learnt, *about* minds. But I will hazard the remark that some of our uses of the word mind suggest to me that in ordinary life we do sometimes think of minds pretty nearly as bundles of dispositions, tendencies, etc. On the other hand, certain other uses certainly suggest that we sometimes think of minds as unique unities—perhaps as "organic unities" displayed in the exercise of different dispositions. Be this as it may, the metaphysical issues here involved do not affect the methods and *rationale* of the branches of psychology I have been considering. If, however, there should turn out to be psychological questions regarding minds *qua* unique or organic unities—or as regards the different kinds of organic unity that different minds display—these would have to be approached by quite different methods from those I have just discussed. And I believe that there are such problems, relating to so-called "theories of psychological types," and that the proper methods for dealing with these remain to be discovered.

II. *By* W. J. H. SPROTT.

No one will deny Mr. Gallie's contention that the language of psychologists is frequently ambiguous. This is partly due to the fact that the psychologist is bound to use words which have been rendered imprecise by popular usage, and words, furthermore, which are all too frequently metaphorical, being drawn from a linguistic primarily concerned with reference to the outside world. A further and systematic ambiguity is added by the fact that several words refer indiscriminately to a psychological phenomenon and to its product : *e.g.*, "judgment," "solution of a problem," "choice." The proposition "he knows . . ." is even more complicated : it may refer to a disposition, in which case it may be true, whether "he" is "knowing" *now* or not ; it may refer to a logical commitment, as Mr. Gallie suggests, in which case it has, so far as I can see, nothing to do with psychology at all ; it *may* refer to an experience of "knowing" *now*. It is not surprising, therefore, that the commonsense word "act" is ambiguous.

As a rough and ready description of psychology, I would say that it is the study of the behaviour and experiences of persons, provided that the word "behaviour" is taken to cover mental as well as overt behaviour. The psychologist attempts at least three things : description, the construction of frames of reference, in terms of which behaviour is interpreted, and the establishment of causal relations and uniformities. This analysis is, of course, artificial ; the three processes : descriptions, interpretation and the establishment of causal relations, are not separate tasks, but rather aspects of psychological research, but I want to ask Mr. Gallie's questions with reference to each of the three aspects I have differentiated.

(1) *Description*.—A great deal of the work of psychologists is concerned with description, analysis and differen-

tiation ; indeed, I think it happens all too often that a description passes muster as an explanation. However that may be, we should all agree that the psychologist (*a*) classifies experience into such categories as "sensation," "perception," "imagery" and the like ; (*b*) analyzes and differentiates behaviour into such categories as "habit," "solution of a problem," "reflex" (simple and conditioned), and so forth ; and (*c*) makes the important distinction between what appears to be passive reception and response, and what appears to be "active" concentration of attention, or intentional behaviour. How far, in his descriptions and analyses, does the psychologist study mental acts? Mr. Gallie would seem to agree that the psychologist should study "mental action, working, performance, etc." but he contends at the beginning of his paper that "in so far as psychology studies things mental, or minds, it never does this through an investigation or analysis of mental acts."

It is not always easy to see what Mr. Gallie would require of the psychologist as evidence that he *does* study mental acts. Sometimes it seems as though his view is, not so much that the psychologist does not study mental acts, as that when he does study them he does so to no good purpose.

Whether the results of his study are valuable or not, I contend that the psychologist does study mental acts in some sense of the word "study," and in both senses of the expression "mental acts" which I shall attempt to distinguish.

In describing and analyzing psycho-physical behaviour, it soon becomes clear that a false account is given if one splits up whatever process one is investigating into discreet parts, because the describable components derive their significance from their functional position in a larger whole. We attempt to cope with this by free use of such expressions as : "organic unities," "integration" or "configuration." Sometimes the assertion of a whole that it is a "configuration" rather than a "mere sum" is based on causal considerations, *e.g.* that a change in the stimulus responsible for one "part" will bring about a change in the effects of other "integrated" stimuli, but sometimes one cannot

avoid in description itself the reference to a complex process as one process, and, using the language of common sense, one is tempted to call the whole process one mental "act." In the solution of an intelligence test a mental "set" may be established by the instructions, the crucial relation may at first spring to the eye when the relata are perceived, and the required correlate may dart into the mind without reflection. But—and this is the point—I may reach a pair of relata between which I do not perceive the crucial relationship, and then I become active and search. Whatever we may say about mental acts *before* this difficulty arose—and I am not concerned with the propriety of calling whatever took place "mental acts"—*now*, in searching, I am certainly being active, and I do not think it misleading to describe my "searching" as *a* mental act. A great deal happens within the compass of the "act"—suggested alternatives, new ways of looking at the standard relata, and so forth, but the whole process is one, from search to solution, and it differs from what was going on before in being accompanied by the experience of active searching. Furthermore, I do not see why Mr. Gallie should not grant me this, because it has the characteristics which, in his opinion, merit the ascription of the word "act" to reflex acts: (a) it is "a more or less isolable phenomenon"; (b) its last phase "has a fairly fixed *temporal*" (not spatial) "relation to its first phase"; and (c) while I agree that it is not true to say that "every phase of it can be linked back causally to a first phase," nevertheless there is *some* causal relation between the initiation of my search and the items which come into my mind as I proceed to the solution of my problem. I want to call these "long-drawn-out acts": "persistent mental acts," to distinguish them from "atomic acts," which I shall discuss below.

Now the study of what I call "persistent mental acts" is of great importance. By such an "act" we can decide to look at an ambiguous figure "as" this rather than "as" that. In the January (1947) volume of the *British Journal of Psychology*—to give another example—Dr.

Himmelweit reports her investigation into the influence of the conscious setting of a level of aspiration on the performance of a task, and she describes an investigation into the way in which neurotics differ in the levels of aspiration at which they aim.

I hold, therefore : (1) that these processes are acts ; (2) that psychologists study them ; and (3) that they are *prima facie* " mental acts." I should like, at this juncture, to refer to one of Mr. Gallie's subsidiary questions : " Does the description of certain mental phenomena as *acts* serve to bring out features of those phenomena that are psychologically important, *i.e.* such that psychology does try to analyze and explain them ? " He emphasizes the word *mental* ; so, indeed, do I. The study of these " persistent acts " may be a crucial matter. Let me give an instance. The Gestalt psychologists, holding their principle of " isomorphism," believe that they can give a complete account of what happens " in the mind " in terms of tensional systems in the body. According to them, I see an ambiguous figure as I do see it because a certain system of electrical potentials has established itself in my brain. If, therefore, I can *decide* to see it " as " something else, and thus, by what appears to be a " mental act," change the physiological configuration, then they have to introduce a new factor into the physiological field, or else confess that a " mental act," which is not isomorphically represented in the neural sectors involved, can have a causal influence over the tensional systems of the body. The same is true whenever what appear to be " mental acts " interrupt what might be expected if the subject were " passive," and the " mentality " of the act instantly comes to be a matter of importance.

Turning now to what I have called " atomic acts," by which I mean, not long-drawn-out unitary processes, but—to borrow a phrase from Mr. Gallie—" pin-pointed " acts. Is the psychologist interested in them ? Mr. Gallie admits that psychologists have, indeed, attempted to focus an analytic eye upon " acts " of judgment. There has, in fact, been a considerable amount of work done in this

field. "*Bewusstseinslagen*" have been explored, "*Aufgaben*" have been set, and the "feel" of solution has been submitted to introspection; acres have been written on the matter, culminating in Titchener's "*Psychology of Thought*" in which he observes that: "While the analysis and localization of my particular feeling of 'but' has value only for individual psychology, I do roughly localize it and I can roughly analyze it into its constituents," and his analysis is extremely odd. Ever since he sat behind a somewhat emphatic lecturer, who made great use of the monosyllable "but," he says: "'my feeling of but' has contained a flashing picture of a bald crown, with a fringe of hair below, and a massive shoulder, the whole passing swiftly down the visual field from north-west to south-east." I am inclined to agree with Mr. Gallie that these introspective analyses are not generally illuminating, that subjects differ in their reports, and that on the whole we all know how to identify the "acts" in question in our own experience. But, for all that, and in spite of the systematic difficulty of introspecting without altering what you are engaged in studying, the investigation and analysis of mental acts still goes on. In the same volume of the *British Journal* to which I have already referred there is an account of an investigation into the recalling of thoughts. The writer wanted to find out whether you could recall a thought without its being formulated in words. He proceeds by introspection, and asking others to introspect. It is a perfectly legitimate investigation, the "act" of recall is, I would say, certainly mental, and, again, the "mentality" of it is important, because if such an act could take place without any verbalization then whatever hypothesis were put forward as to the relation between the "mind" and the "body" would have to be adjusted to that fact. But Mr. Gallie sets us a psychological problem of this kind himself. He distinguishes between "knowing" and "judging." His distinction seems to me to be non-psychological, and to be concerned with the logical implications which *ought* to be observed by persons who "know" or "judge," and it is noticeable that at one point he slips

in the parenthesis "(if I have a logical conscience)". This seems to me to imply that what *ought* to happen when we "know" or "judge" (which is the business of the logician) does not always, in fact, happen. It is not, however, merely to complain that Mr. Gallie has introduced an epistemological red herring into his argument, that I refer to his discussion of so-called "acts" of judging and knowing. He has, I think, raised and ignored a matter of importance in this connexion. He says: "We all know already how to identify" acts of judging and knowing, etc., "in our own experience: thus, I know that I know . . ." How? "When, for example, I possess an answer which, on whatever grounds, I consider conclusive to a given question." That word "*consider*" in its context might be replaced, I think, by the word "judge." Does this mean that when he says "I know . . ." there are occurring judgments that certain propositions are conclusive answers to a question? If so, these judgments might surely be at fault, and would he not, in that event, "know" that which was false? For example, if I consider the conclusive answer to the question "How old is Mr. Gallie?" to be "He is 34," on the grounds that he mentions that age in his paper, would Mr. Gallie say that I "*know*" that he is 34 years old? I doubt it. I do not wish to pursue this question; I am not competent to do so. I would rather ask whether Mr. Gallie distinguishes his "knowings" from his "judgings" in any introspective way? Doubting and supposing "feel" different from one another, and both "feel" different from judging, but does judging "feel" different from knowing? I am not interested at the moment in the answer to this question. All I want to point out is that Mr. Gallie has, doubtless unintentionally, caused me to raise an issue which involves the scrutiny of the mental acts of judging and knowing (if any).

I have spent most of the time trying to deal with the way in which psychologists are concerned with mental acts when they are mainly occupied with the description and analysis of what happens in the field they are investigating. I have done this because it is obviously with the

descriptive aspect of psychological research that we are primarily dealing, when we ask whether psychologists study mental acts.

The construction of interpretive frame-works.—It is true that many psychologists—perhaps the majority—are interested in the construction of interpretive frames of reference, by which I mean any dispositional concept. Intelligence, habits (when the word is used to refer to the *likelihood* of a person responding in a certain routine manner, and *not* to the processes themselves, which are instances of, or evidence of, “habits” in the dispositional sense), characters, complexes, instinctive tendencies, sentiments, traits, functionally autonomous motives, attitudes, etc., are all instances of such “interpretive frameworks.” I agree that the focus of interest is the dispositional scheme, rather than the actual manifestation of it in operation. A subject’s actual performance in a test is not *per se* interesting ; it is the mental capacity which is inferred which takes the centre of the stage. Similarly, the gesture of politeness is not studied, it is the trait of politeness which is important—not what the subject does *now*, but what he is likely to do under other circumstances. Even here, however, we must beware of too sweeping a statement. It is, after all from a study of mental acts that these constructs are elaborated—that, I think, Mr. Gallie, would admit, but he says that the mental acts are only of minor significance *per se*—that I would admit. There are, however, occasions, when the mental acts come in for closer scrutiny. There used to be a type of intelligence test in which the testee was asked to indicate “the best answer” to a predicament, or the “best reason” for a statement. One of these runs :

“When a little girl loses her doll, she should :

A. Cry till somebody finds it for her.

B. Think where she is likely to have left it and look there.

C. Search in her father’s pockets.

D. Ask her mother to buy her a new one.”

Now all these answers have some plausibility, and if any one is given we cannot infer intelligence, cynicism,

worldliness or stupidity or anything else with any degree of probability. This means that it is a bad test. From this criticism it is clear that we reject the test on the grounds that we cannot choose between the possible mental acts which could be inferred from any given performance (a performance being the writing down of one of the letters A, B, C, or D). Now, surely, this means that in the construction of an intelligence test we cannot help reflecting on the *sort* of mental act which is required for the solution.

Similarly, I would say that the psycho-analyst is certainly interested in the mental acts of his patients in so far as they may be taken as symptoms of the inferred entity, which he is really trying to detect, and he is not *usually* interested in them *per se*. But let us suppose that a patient observes: "I hate your beard!" I submit that the analyst cannot instantly take this as a symptom, or rather that he has to scrutinize it before he decides what it is a symptom of. Is the patient pulling his leg? Is he making an aggressive gesture? Is he saying this because he thinks that at some time during an analysis such observations are expected? Of course, the answers which will be given to these questions depend, in part, on the "interpretive frame-work" which the analyst has already built up—his idea of the "character" of the patient, and the way in which his remark fits in with other remarks and remembering, and so forth, but he will use all this material to answer the question, "What mental act did my patient perform?"

The establishment of causal relations.—Does the psychologist study mental acts as such when he tries to establish causal relations? I have already suggested that he does. When, in a large-scale process, such as the perception of an ambiguous figure, or the completion of an intelligence test, the mental act of deciding to see the figure this way rather than that takes place, or a mental act of searching is initiated, the psychologist may be interested in the mental act (a) as a *prima facie* mental intrusion into a process which he may think he can explain in terms of physiological causation; (b) as an intrusion which he has to account for; and

(c) as an ostensible cause of what follows : the seeing of the figure in the intended way, or the achievement of a solution by means of a search. This may be illustrated by a story told by Woodworth of an unfortunate student who was supposed to be memorizing nonsense syllables. "After the list had been passed before him many times without his giving the expected signal that he was ready to recite, the experimenter remarked that he seemed to be having trouble in memorizing the syllables. 'Oh ! I didn't understand that I was to learn them,' he said." The implication is that the presence or absence of a mental act—a "persistent mental act," as I should call it—of intending to memorize makes a difference to the proficiency of the memorizing. You may say that we all knew that before, and this may well be true, but the point is that the registration of what might be called "intrusive mental acts" is of the utmost importance when it comes to the causal explanation of behaviour. Perhaps, indeed, that is why some psychologists do not pay them the attention that is their due, because it is precisely this kind of phenomenon that makes behaviouristic and reflexological explanations so unpalatable. It is, for instance, surely significant that in order to establish a conditioned reflex in a human being you usually—if not always—have to invite your subject to adopt an attitude of passivity. The investigation of conditioned reflexes in the artificial aspic of a peaceful laboratory is of interest, of course, but it does not throw as much light on human behaviour as is sometimes thought, because, I would say, "mental acts" keep on breaking in, and it is their *prima facie* mentality that causes us so much bother.

To sum up : I agree that we, as psychologists, ought to keep a more watchful eye than we do on our terminology. I agree that most of us are concerned with dispositions rather than with what is happening "in the mind" here and now. But I think Mr. Gallie has rather more than over-stated his case. I have suggested that we study mental acts directly in our description and analysis of psychophysical behaviour, that we have to scrutinize mental acts even when we are not primarily interested in their analysis ;

and that the registration of "interruptive" mental acts is causally important precisely because they are mental.

I have made no reference to the obvious importance of acts of choice—whether they occur, what is their cause, and what causal efficacy they have? I have concentrated on what I think are obvious mental acts, and have ignored the question of whether there is mental activity going on even when we appear to be passively perceiving or day-dreaming, as distinct from the "active" processes of attending and thinking. I do not pretend to have answered all Mr. Gallie's questions. I think that psychologists *do* study mental acts; I think that "the description of certain mental phenomena as acts" *does* "serve to bring out features of those phenomena that are psychologically important; I think that "when psychologists properly describe certain objects of their study as acts" they may very well be "concerned to show that such acts are *mental* or acts of minds"; and, finally, that at any rate the *recognition* of such acts as mental "is relevant to the character of their investigations." To which I would add that the psychologists may do well to be aware of the possibility that such acts are not merely mental, but may be of such a nature that no physiological correlate can be found for them.

III. *By* C. A. MACE.

IF it were the duty of the third contributor to a symposium such as this—in which a sharp divergence of opinion has emerged—to attempt to mediate or reconcile, I should find myself in a difficult position ; for my first reaction to the two foregoing papers is to say that I disagree with Mr. Gallie and that I agree with Mr. Sprott. I think Mr. Gallie is mistaken in denying, and that Mr. Sprott is right in affirming, that psychologists study mental acts.

But this admittedly is only a first reaction, and one that I do not wholly trust because of a curious feeling that, whilst I am pretty sure that I agree with Mr. Sprott, I am not so sure that I disagree with Mr. Gallie. From this, I suppose, it follows that I am not entirely certain that these two symposiasts really disagree with one another. Mr. Gallie raises an immense variety of issues. On some I certainly disagree with the views that he expresses. On others I agree with what he says. But on certain crucial points I confess I am not wholly clear as to the position he is defending.

I propose, accordingly, first of all to state in my own way what I take to be implied by the assertion that psychologists do study mental acts. I shall then discuss some of Mr. Gallie's statements in order to try to ascertain whether they do in fact throw any doubt on what I take to be the position of Mr. Sprott and myself. I shall conclude by indicating one point at least in which I think I am in agreement with Mr. Gallie.

I.

In the olden days, people who described themselves, and were described by others, as psychologists applied their minds to questions such as this : Can we will to do something that we believe to be quite impossible ? On the face of it, to raise such a question is to study mental acts.

To make up one's mind to do something is surely a mental act, and to inquire into the conditions under which such acts are possible is surely to *study* these acts. To reach agreement on this it is not necessary to consider all the ambiguities in the expression "mental act"; and we do not need, for this purpose, to define this expression. We can agree, I should have thought, that there is, among all the senses of the phrase, one understandable sense in which a decision is a mental act; and if we tried to define all the senses of the expression we should be bound to include this case. We should be bound to frame a definition which satisfied the condition that it did, in fact, apply to the making of decisions.

If, then, we can, in fact, observe a psychologist actually making an inquiry of this kind we have established the conclusion that psychologists sometimes study mental acts.

Admittedly, there are several things that are rather curious in some of these inquiries. One way in which the question whether we can will to do what we believe to be impossible has been tackled was to contemplate very deeply some selected, and generally hypothetical, case: *e.g.* Can I will to jump over the moon? It was part of the technique to select extreme and rather quaint examples. Then, having contemplated one or two cases of this kind, the thinker would announce his answer. It was common, too, for such inquiries to be conducted whilst the thinker was reclining in an arm chair; and this part of the technique has been a matter of profound and persistent reproach.

Later, however, inquiries of this kind were conducted in a laboratory. The thinker would then be seated on a hard stool and someone with a stop-watch would record the time it took him to make up his mind.

Some would assert, and some deny, that the two procedures differed in a manner that is scientifically significant; but whatever may be the truth about this, there is little doubt that both procedures are very different from those most commonly followed by psychologists to-day. Contemporary technique can best be illustrated by one or two examples.

There is a well-known experiment of Lewin which was conducted in the following way. Lewin gave his "subject" the impression that the experiment was a study of fatigue. The subject was instructed to sign his name over and over again until a point was reached at which he felt it to be literally impossible for him to sign his name again. When the subject said, "I can't go on, I can't write another word," Lewin said, "All right, you can stop now. Just put your name on the top of your paper and hand it in." This the subject did with cheerful alacrity. Apparently, he willed to do, and actually did, what he had declared to be impossible.

There is, of course, more than one way in which the result of this experiment may be interpreted, but this fact does not affect the points with which we are now concerned. One point to note about contemporary technique is that the psychologist in studying acts of will does not concern himself with extreme and hypothetical examples, but with situations that actually arise or can be experimentally arranged. Another point is that part at least of what is included in the conclusion could not have been predicted merely by contemplation. The conclusion depends in parts upon the observation of facts in the experimental or some similar situation.*

There is another difference between the older and the newer techniques which is brought out by one of Mr. Sprott's examples. Dr. Himmelweit's study of "levels of aspiration" starts from familiar facts: When a person undertakes almost any task he sets himself some sort of objective, defines for himself, clearly or vaguely, implicitly or explicitly, some standard of achievement. If his performance can be scored, he may say to himself, "I will try to reach a score of 50." Some people aim high, some low; but there are conditions affecting everyone alike.

* Reference must be made to some 'similar situation' since many people would say that what Lewin observed was precisely what one would expect. The expectation however, arises, not from simple contemplation but from some prior observation.

How high one aims depends upon what one believes one can achieve. What one believes about this, and how firmly one believes it, will be affected by past experience in this or similar tasks. If at the last attempt one aimed at 50 and scored 60, one may come to believe that 65 is attainable and go for that score. If one had aimed at 50 and scored only 30, both the belief and consequent intention will be correspondingly affected.

From the study of such matters evidence is obtained for general statements, such as the following: Successful performance raises the standard aimed at, failure causes the standard to be depressed; there is a general tendency to over-estimate future performance and to under-estimate past performance. Women tend both to over-estimate future performance and to under-estimate past performance less than men. Normal subjects tend to under-estimate their past performance more than do neurotics. Himmelweit's experiments, like Lewin's, were concerned with actual mental acts, with actual judgments and actual intentions, and they elicited facts not all of which could have been predicted by contemplation without observation. But her experiments illustrate more clearly than the experiment I have quoted from Lewin another characteristic feature of contemporary psychological procedure. The empirical psychologist treats the things he observes as variables. An intention varies in respect of what is intended, in respect of its vagueness or determinateness, in its "intensity" and in other ways as well. So, too, beliefs vary in respect of a number of attributes.

A great deal may be discovered by observing the changes that occur in what a person decides to do according as circumstances affect the assurance of the beliefs that are relevant to this decision. Much more is discovered by the systematic variation of these conditions than by contemplating extreme or limiting cases.

Such questions are, in fact, studied, and I cannot see that Mr. Sprott and I are saying anything obscure, dubious, controversial or misleading when we state the fact in general terms by saying that psychologists sometimes study mental

acts. It seems less misleading to say this than to say that they do not.

So clearly does this seem to me to be the case, that I find it difficult to believe that Mr. Gallie wishes to say otherwise.

II.

It is possible that Mr. Gallie does not wish to disagree with the statement that psychologists study mental acts in the sense in which I, and I think Mr. Sprott, would understand it. There are several passages in his paper which suggest that the issue in which he is chiefly interested is one of an entirely different kind.

There is, however, one passage in which he refers to inquiries similar to those of Himmelweit and Lewin. He writes: "It might be claimed that psychology studies mental acts . . . in respect, not of their inner natures but of their causes and effects." This is precisely what Mr. Sprott and I do in fact claim, and Mr. Gallie's attempted rebuttal of the claim does not greatly disturb me, because he considers only a rather special (and I should have said not a very plausible) account of the connexion between an act and its conditions.

But even in this passage there are indications that the question he is here discussing is secondary in his mind to another of much greater interest, and one which has little connexion with the position that Mr. Sprott and I are attempting to defend. What I suspect interests him most is the suggestion that psychologists study mental acts *in respect of their inner natures*, and this expression perhaps requires to be taken in conjunction with a qualifying phrase that occurs to his initial formulation of his central thesis. In the opening paragraph of his paper, he says that in so far as psychology studies things mental or minds, it never does so through an investigation or *analysis* of mental acts. And there is a third statement which I think may be intended to carry similar implications. He asks: Does psychology study acts which can be identified as acts of knowing, judging, etc., and study them *in respect of their*

being such ? And he replies : “ I don’t think it does. For one reason I don’t see that any useful purpose would be served by a psychological study of such acts. We all know how to identify them in our own experience.”

At first sight this appears a queer argument. What should we say to a zoologist who said : No useful purpose would be served by a study of elephants and tigers because we can always identify them without difficulty. And what should we say to a scientist who said : There is no need to study the difference between the act of hopping and the act of running because we all know when we are going on one leg and when we are going on two. But this reply would be superficial. Mr. Gallie would, I think, be right in saying that the cases are not in the least parallel. There are observations which a zoologist can make in regard to tigers which will tell him something about the “inner nature” of a tiger ; physiologists and anatomists can make observations which will yield new knowledge concerning the inner nature of the act of hopping. Mr. Gallie doubts whether a psychologist can make corresponding observations which will reveal the inner nature of a judgment or a decision, and I share his doubts.

I began by stating, in my own way, the sense in which I believe myself to be in agreement with Mr. Sprott. I will conclude by stating in my own way a point of view which may be in close agreement with that of Mr. Gallie. But I am very much less certain that what I have to say will be acceptable as a concession to Mr. Gallie’s thesis. It may be wholly irrelevant to what he has in mind. But it will, I trust, be a partial attempt to meet his challenge to psychologists “ to define their aims and methods.”

III.

I have referred to three phases in the history of the study of mental acts. My reference was brief and inadequate, but what I chiefly wished to suggest was that psychologists have encountered the most perplexing problems through the failure to distinguish with sufficient clarity several different sorts of questions and the different procedures

through which these questions may be answered. There are, I think, three kinds of inquiry, each of which might not wholly inappropriately be described as a study of mental acts.

The most straightforward of these inquiries is that of the kind which I have tried to illustrate by reference to the experiments of Lewin and Himmelweit. This is the case in which an experimentalist observes the reactions of some other person under controlled or variable conditions. In respect of methodology this case does not differ in any important respect from inquiries pursued in other experimental sciences. The second sort of inquiry is of a kind which can, I think, be properly described as philosophical. This is illustrated by the question, "Can we will to do something that we believe to be impossible?" When this question is interpreted in a sense in which the answer does not depend upon the result of an experimental observation, but is answered by some kind of process of "analysis," in consequence of which the conclusion will be reached that "willing to perform p " does or does not *entail* "believing that p is possible to perform."

As to the nature of the analytic procedure we know philosophers differ. Some would say that it consists in analyzing "concepts," others that it consists in examining the way in which words are used. I need not re-open the question here, but will content myself with an observation regarding the relation of this sort of problem to Mr. Gallie's thesis. If this kind of inquiry regarding "willing" and "believing" may be described as a study of mental acts, we might be tempted to answer Mr. Gallie's question by saying: No, psychologists do not study mental acts in this sense, but philosophers do. But this can be said, I think, only with certain reservations. The problem is philosophical, but it is one to which the psychologist also must pay some attention. Psychologists have been muddled, and are always liable to get muddled, through the confusion of these different kinds of question. This liability arises not from the weakness of their intellects, but from the inherent nature of their job.

Among psychologists, E. B. Titchener is outstanding as one who has made the most systematic attempt to disentangle the empirical problems of psychology from the more strictly philosophical issues. Psychology, he said, is concerned with "existences," philosophy with "meanings." His treatment of the matter is not, I think, entirely satisfactory; but it affords a convenient point of departure from which to distinguish a third kind of problem with which the psychologist is concerned, one that differs both from problems of analysis and from experimental problems of the type that is common to all the empirical sciences.

Titchener was trying to distinguish questions of philosophical analysis not so much from questions which are answered by the observation of one person by another, but from questions which are answered by one person observing himself. He was concerned, in other words, with a certain procedure traditionally described as a process of "introspection." Now I strongly suspect that some of Mr. Gallie's perplexities concerning the study of mental acts arise in regard to this alleged procedure. And well may Mr. Gallie be perplexed. Traditional accounts of introspection probably contain more examples of the confusion between observational and "analytic" procedures than any other topic in psychology. They involve the additional complication that the acceptance of statements concerning introspection is commonly intended to imply the acceptance of special and often extremely dubious theories, such as the theory of philosophical dualism, and the theory that in introspection we employ an entirely unique and distinctive mode of observation. All this has led, as we know, to destructive criticism of the concept of introspection in philosophy and to a widespread tendency to discard it as a psychological procedure. This, I believe, is the wrong solution. The position I would defend is that there is an observational procedure for which the term "introspection" can be usefully employed.

The psychologist who studies human beings is in the distinctive position that he can obtain certain data by observing himself. He can observe himself in a state of emotional

excitement, or in the process of coming to a decision or of making up his mind. What is important in this is that very often, though, of course, by no means always, it is easier to observe these things happening to himself than to observe them in another person. His own decisions, emotions, and beliefs have often a greater accessibility than the decisions, emotions and beliefs of other persons.

We can I think conveniently use the word "introspection" for the observation of states and process in oneself which have this ready accessibility and we can so use it without any theoretical implications. Whether observing oneself in this way differs "in principle" from observing another person is a question concerning which nothing need be initially assumed, however much it may invite further investigation.

If I had to state a *theory* concerning introspection I should try to develop it along the following lines. I take the common view that the "immediate" data of observation are "objects" of the kind variously described as "sense," "sense data" or "experiences" through which we may be said to observe the physical world. In introspection, I should say, we are observing our own bodily states and processes. Thus, to take an easy case, when I attend to my toothache I am, in a way (and on typical occasions) observing a state of my tooth. When the dentist examines my tooth he observes it through visual data. I, on the other hand, am attending to sensory data of another "modality." His data are private in one sense, mine are private in two. If he wants to confirm his observation he may ask the anaesthetist to look. The dentist's sense-data are private; so, too, are the anaesthetist's, but confirmation is effected through the systematic relations between the two sets of data which can be expressed in public language. My non-visual data are private in the sense in which the dentist's data are private. They are private also in the sense that I cannot ask anyone else to feel the pain. There is nothing surprising or very important in this. The anaesthetist's confirmation of the dentist's observation does not depend upon the fact that both are visual data, but upon

the "systematic relations." Such systematic relations may hold between data of differing modality; and this applies when one set of data happen to be private in the second sense. So the dentist and I can check each others statements. He may say that the judgment I made on the basis of my data is wrong. "There is nothing the matter with that molar, the trouble is in the incisor," he may say, and say correctly. If he says "There is nothing wrong at all, its all imagination on your part," I can properly retort: "I know its not imagination, there must be *something* wrong *somewhere*." In all this kind of conversation it seems clear that I am relying on observations that are relevant to the matter under discussion, and that I have data that are not accessible to the dentist. In point of fact dentists and doctors do sometimes listen to what their patients say. What applies to observing *states* of ones own body applies also, I suggest, to observing ones own acts. Anyone can observe some of my acts of decision, and they may do so by noting my grosser movements, and such things as the changing expression in my eyes and the changing tensions in the muscles of my jaw. I have corresponding data. I can feel my grosser movements and the changing tensions in the muscles of my jaw; and I can, so to speak, listen to myself talking to myself when I tell myself what I am going to do. Each of us is in a position permanently to eavesdrop on our own inner conversations, and to spy upon our own inner movements. Maybe this is all that introspection is. If so it is one of the special cases in which we attend to sensory data other than the visual data upon which scientific observers usually but not necessarily rely. But this is *theory*, and it may be all complete nonsense. Its falsity would not affect what I take to be the fact that we do in some way observe our own mental acts.

It remains to take up Mr. Gallie's query whether "any useful purpose is served" by the attempt through introspection to study our own acts.

I should say there is. I should say that we can find out quite a lot about mental acts by observing them as we perform them ourselves. I base this partly on their ready

accessibility. For example I think we might have discovered that a change in the way in which we think of a task will affect our beliefs in the possibility of the performance and consequently the decision to perform it without subjecting some unfortunate individual to the kind of ordeal to which Lewin subjected his subject. When we read the reports of psychological experiments we often feel that the result obtained is only what we might have expected ; that they do not tell us anything that is surprising or new, they merely *confirm* antecedent expectations. Many of these expectations, I suspect, have their origin in previous experience of our own reactions as much as in previous observations of the behaviour of other persons. It may be that self-observation is the most useful source of hypotheses, and that the chief value of experiments is to test such hypotheses. If so, introspection would be quite important. Whether we ever discover anything by observing ourselves which we could not in principle have discovered by observing other persons is another question which we may leave open.

On the side of observing oneself there is the advantage of easier accessibility of the data, on the side of observing others by experimental method there is the advantage of conclusiveness and precision and statistical refinement.

On the question whether either procedure has any advantage in respect of observing the "internal nature" of an act I confess I do not know what to say. I assume that what Mr. Gallie means by the expression is what might also be expressed as "the intrinsic nature" of the act. I cannot see any reason for saying that the intrinsic nature of an act is more clearly seen when we observe ourselves perform it than when we observe someone else perform it.

I think I should say that psychologists are not concerned with the intrinsic nature of acts, and that they do not study acts merely by contemplating them as they are in themselves. Their job is to study the relations of acts to one another and the relations of acts to other things. In this I hope I am in agreement with Mr. Gallie.

THE PROBLEM OF GUILT.

MR. H. D. LEWIS, MR. J. W. HARVEY AND MR. G. PAUL.

I.—*By* H. D. LEWIS.

MORAL philosophers do not seem to have had a great deal to say about the problem of guilt, and it would be easy to compile an impressive list of ethical treatises in which the problem is not mentioned at all. In recent ethics especially it has suffered much neglect. In theology, on the other hand, the problem has always remained to the fore, and of late it has also elicited the very lively interest of the psychologist. It is the moralist who remains aloof.

This is as regrettable as it is strange. For however important the problem of guilt may be, in some of its bearings, for religious thought or psychology, it is first and foremost an ethical problem. And when the moralists are reluctant to tackle some ethical question, and are content to hand it over to other disciplines, such as theology or psychology, which have an interest in it, the properly ethical features of that question are apt to be either overlaid altogether by extraneous considerations or distorted into some quasi-ethical religions or psychological form. Of this the treatment of the problem of guilt is an excellent example.

We have thus to insist at the start that our problem is essentially an ethical one. It is also a problem of first importance.

This is so for several reasons. (1) Some of the most crucial issues in theological controversy have turned on the nature of guilt, and the course which the discussion of these matters has taken has been such as would have been substantially improved by bolder intrusion into the field of theology by moral philosophers—especially in recent times. (2) The rapid advance of psychology in our time, and in particular the fascinating, if also somewhat uncritical theories of the psycho-analysts, have added much to the plausibility of the view that a fully satisfactory account of

guilt can be given in purely psychological terms ; this alone would justify a fresh investigation. (3) Questions of penal reform and other matters of even more urgent and far-reaching importance, such as the problem of war-guilt and the treatment of " guilty nations," set our problem among the more pressing practical issues of this age. There is hardly another field where the philosopher, by close and impartial analysis can have a more immediate and beneficial influence on practice, nor any where the challenge to come out of the ivory tower is plainer. (4) Some of the matters most keenly debated in ethics to-day, and especially the question of rightness and moral worth on which so much ingenuity has been expended, could have been viewed more clearly from the start if the implications of guilt, as involved in the nature of moral evil, had been more carefully and boldly considered in connection with those matters. [This, although not taken up very fully in this paper (I have discussed it elsewhere) will, I hope, be plainer in due course.]

We have thus a vast field of investigation before us, and it will only be possible to touch very briefly on some of the matters mentioned above. My aim will be to centre attention as far as possible on the strictly ethical nature of the problem of guilt.

II.

What, then, do we mean by guilt ? A person becomes guilty when he has done what is wrong—for example, in law, when he has contravened the " law of the land," and incurred a penalty. But there are several senses of " doing what is wrong " in ethics. Sir David Ross distinguishes three :* (A) my act may be wrong by being out of accord with the actual requirements of a situation, or (B) by being out of accord with the requirements of the situation as I understand them, or (C) by being contrary to what I myself take to be my duty. These distinctions are required because we may be mistaken both about the facts of a

* *Foundations of Ethics*, Ross speaks mainly of " right action " but what he says obviously holds *mutatis mutandis* of " wrong " action.

situation and in our evaluation of them, and they are sufficiently familiar distinctions not to call for close comment here. Is guilt, then, incurred by doing what is wrong in any of these senses? Not, it seems plain in senses A and B. For failure in these respects reflects no discredit directly on the agent, and that seems to be plainly involved in the idea of guilt. What, then, of C? Here Ross's position is peculiar. For one of his main reasons for regarding C as the most important meaning of a failure to discharge our duty (and that in which "obligation" may be substituted for duty) is that it is the one in which blame is incurred, and blame is incurred because we *can* "set ourselves" or intend to do what we think is right, whereas we cannot guarantee the result. But Ross does not think that rightness or wrongness in the present sense is a direct indication of our moral quality. He argues that we may do what we think is right merely to suit our convenience or from thoroughly unworthy motives, and that our conduct would, then, be morally indifferent or bad, as the case may be. When we do what we think is *wrong* there is *some* evidence of moral wickedness, but motive must here also be taken into account in a complete evaluation. But it seems to me certain that conduct does not deserve blame except in strict proportion to moral disvalue, and that no one is guilty in the ethical sense except as he is also morally bad. Can we then equate guilt with moral evil, and simply ask by what sort of wrongful action is such evil incurred, and how this bears on Ross's distinctions? One is much tempted to say "Yes." But that would not, I think, be quite correct. What, then, more exactly, must we understand by guilt?

As suggested already, what we associate with guilt is blame, condemnation, remorse, and righteous indignation. On some views punishment is also directly involved, but this raises some further, rather complicated, questions, and I leave it aside for the moment. But whether we have punishment in the picture or not, we can hardly think of guilt without thinking in some way, of blame and condemnation. But what do these mean, and how are they related to guilt? Can we define guilt in terms of them?

If we could straightway do that our ethical problems would be very much simplified. For that would mean that these reactions could themselves be exhaustively described in psychological terms, and this, in its turn, would give us the main indication of the nature of moral evil which would also need to be naturalistic ; from this, as we shall see below, would result a fairly easy solution of the problem of freedom. But this is not what we normally think. We think normally and, I believe, rightly, that blame and condemnation do not define moral evil, but rather presuppose it. They are attitudes and reactions appropriate to such evil, but the moral evil comes first. This means that the latter is an ultimate ethical conception not reducible to factual terms. Moral blame and condemnation therefore derive their significance from it, not it from them. They are attitudes which have a peculiar irreducible ethical appropriateness to moral evil. And it seems to me that guilt consists in the requiring, by morally evil conduct, of these special attitudes in the way described. As such it does not open up a possibility of defining moral evil, but it aids our reflection about it, in particular by throwing into relief the contrast between moral evil and other forms of evil which do not call for blame. This is why it has special importance for the ethical philosopher.

Mutatis mutandis, this could also, in strictness, be said of merit. But language is apt to be misleading here. For merit is not a term we reserve for strictly moral matters. One may acquire merit and deserve praise as an athlete, a scientist or a poet. But "guilt" is never incurred in any of these respects. For a reason which I cannot fathom, but which I much suspect to be bound up with the forensic form in which the idea of guilt took shape originally, we have retained a distinctive term for that quality of conduct which calls for moral blame, without having a term, restricted in the same way, as the opposite of guilt in its ethical usage. Hence it is more instructive to centre attention on the ideas of guilt and blame and remorse, bearing in mind, at the same time that they have their correlatives.

To return to our main theme, I should like next to enter a vigorous protest against the procedure of several eminent moral philosophers who, without any wish to put ethical ideas into jeopardy, but rather the reverse, have seriously prejudiced their case by defining responsibility as liability to punishment, and guilt as the meriting of it. Bradley is a good example.* The position is in some ways redeemed by the fact that punishment is not itself conceived wholly, or even mainly, in utilitarian terms. There is retributive punishment, the appropriateness of which is distinctively ethical. But the case for retention of guilt as a properly ethical notion is made to depend on what seems to me to say the least a highly doubtful conception, namely that of retributive justice. I believe that something like the theory of retributive punishment may well be true, as I hope to suggest later, but I am certainly far from convinced that any situation is directly improved by infliction of pain on a guilty agent. But this does not vitally affect the problem of moral evil and guilt itself. The question of punishment is a separate issue for any account of it which is not utilitarian. For punishment would have no special appropriateness (allowing for the moment that it has) were it not for the prior irreducible nature of moral evil.

If we overlook this, as Bradley and those who follow his lead are much inclined to do, we may find that we have moved unwittingly near to the naturalist in position, and it is because they have travelled further in that direction than they realized, I believe, that the thinkers in question find it so easy, as a rule, to reconcile freedom with determinism. This is why I think it important to keep the problems of responsibility and guilt distinct from the problem of punishment, except, of course, to the extent that punishment is bound to have a very central place in any naturalistic theory of guilt.

III.

But the view that guilt may be fully described in psychological terms, and that the belief that it has some distinc-

* See *Ethical Studies*, Chapter I.

tively ethical character must be treated as an illusion—in the sense, for example, in which Freud regards religion as an illusion—will have very strong support at present. There is hardly space here to discuss the matter in detail, for it would take us into the general question of naturalistic ethics. But since the idea of guilt has figured rather prominently in some recent attempts to give a psychological account of ethical ideas, some comment is required.

The matter that falls to be emphasized first is the stubbornness of the belief that guilt is real, and that it is not to be described exhaustively in terms of punishment and similar attitudes. Illusion there may be, but it certainly dies very hard. The voice of vulgar opinion, whose prestige is rightly very high in ethics, speaks here in no unmistakable terms. The ordinary person, by no means so elusive a creature as is sometimes thought, draws a very sharp distinction between his shortcomings in art, or in matters of the intellect or good taste, and wrongdoing; and if it be suggested that the judgment to be passed on the latter is illusory, or can be exhaustively described in terms of fear and punishment or similar matters, he will be much offended, taking it as an affront to his dignity as a moral being. He believes that some conduct has the attribute of guilt which implies that it is evil in a deep and peculiarly revolting way. The “guilty” are not merely a menace, they are evil; and the evil nature of their actions is not at all on the level of some functional disorder or disease. It is deep and ultimate.

A reflection of this may be found in the procedure of some philosophers who, not themselves unsympathetic to subjectivist theories of non-moral good, have also held very firmly that moral good and moral evil are *sui generis* and ultimate! This may not be an easy position to defend. But at any rate it bears witness to that deeprooted nature of the conviction that guilt derives its significance from an evil which is ultimate and irreducible.

But someone may object here that the report of common experience is that, on occasion, we *feel* guilty or have a *sense* of guilt; and that might be taken to suggest that guilt

is some feeling of uneasiness or fear due to anticipation—not very explicit perhaps—of some kind of punishment. This is, however, to ascribe far too great a significance to an ambiguous phrase. The consciousness of guilt will normally arouse some distressing emotion, of which remorse is by far the most distinctive. And for the rough designations with which we are normally content the mere emotion may well give the best indication of the experience as a whole. But clearly we could not have guilty feelings unless, in the first place, we thought that we *were* guilty. This does not, of course, prove that guilt cannot itself be described, in the final account, in terms of our own emotions. But it does mean that such psychological accounts of guilt must have room for the distinction between the feelings consequent on guilt and the state which occasions them. And the fact that this distinction has to be drawn robs the popular expression “feeling of guilt” of any immediate support it might give to the notion that guilt can be exhaustively described in terms of our emotions.

Furthermore, “feeling” is itself a term notorious for its ambiguous usage in common parlance. We say “I have a feeling that the weather will clear,” “I feel I ought to visit so and so,” when clearly we mean primarily “I think that,” etc., however much the thought may be prompted by feelings or accompanied by them.

It is not, therefore, so simple a matter to dispose of the belief that guilt is “a terrible reality”—and a properly ethical one. But, it may be urged, it is just here that psychology, and especially recent psychology, comes to our aid. It can be shown that there are many hidden anxieties and fears whose subtle and elusive operations create the impression that there is some distinctive ethical guilt when in fact there is nothing that we cannot, on close analysis, reduce to some emotional reaction. On this basis some thinkers, and especially followers of Freud, have maintained that guilt is to be conceived, in the last resort, as “a need for punishment” induced in part by “conditioning” when certain acts have come to be associated with punishment and we feel a doom hanging over us if punishment is not

forthcoming, relieved when it has been administered. A subtler feature of the same phenomenon is the introjection into our "super-ego" of the relief experienced by those who punish us for offending against them by doing them some harm. This happens mainly in early years, and especially in the relations of children to their parents. It becomes in due course one of the most influential factors in our lives, and affords the clue to many social and pathological problems which we might be inclined to oversimplify if we overlooked the fundamental character of the "need for punishment" and the complicated forms which it takes, for example in the inhibitions resulting from the Polycrates complex, or in the operation of vicarious punishment in cases where we have projected our own guilt on to others.

Now it cannot be denied that recent psychology has taught us a great deal in these ways about ourselves. Not that we must straightway endorse its main conclusions, even when presented without any ethical implications. For there is much to invite serious criticism both in the procedures and the findings of psychology, especially of the psycho-analytic type. An exaggerated and somewhat distorted importance is ascribed, for example, to the vicissitudes of our lives in infancy as a clue to later and more mature states. No doubt the formative influences of early years need to be carefully studied, and they can throw very special light on our subsequent history. But we must not overlook the fact that there is development, and that mature experiences remain opaque to inspection on the basis of early factors alone. Admittedly, the more elusive elements in the constitution of adult life will often be found in the experiences we are apt to forget most completely, and it is tempting to the psychologist to seek explanation of what he fails to understand in adult experience in matters which are least accessible to our consciousness. But he does so at his peril, however remarkable some of the discoveries obtained in this way may be. For apart from the fact that conclusions about early years must be drawn with the greatest caution, there is a genuine reforming of the main traits in our nature in the continual flow of life from infancy to

maturity. Even in cases of grave maladjustment or mental disorder, the main clue may be found in some event of adolescence or maturity that does not involve the days of our infancy in any special way. Nor is the preoccupation with abnormal psychology, so noticeable among the Freudians, a help to the achievement of a balanced understanding of mature states. In addition there is a proneness to force the evidence into preconceived patterns, as well as some foolishness in the practice of psycho-analysis, resulting sometimes in no inconsiderable harm. But when objections of this sort have been pressed to the uttermost limit, there remain some truly impressive achievements to be put to the credit side of empirical psychology to-day. We have been shown as never before how to look below the surface for a better understanding of ourselves, and the rudiments at least of a new technique are being evolved. And this leaves us with the question whether, cured of its more extravagant and fanciful tendencies, psycho-analysis can provide an explanation of guilt along the lines suggested. While we cavil at much in the present formulation of the explanation, can we accept it in principle?

If we can, then we must be very clear what we are about. For the upshot of our conclusion will be that moral distinctions, as we normally think of them, will have to be adjudged to be quite without substance, and banned for the mischievous confusions they engender. About this there should be no prevarication. There will be no room for ethics in that aspect of it which we ordinarily regard as having most depth and importance, namely the study of obligation and moral good and evil. We must turn from these matters with the firmness, if also with something of the gentle sadness, with which Plato abandoned the poets. Our fondness for a noble illusion must not dim our eyes to the course we must take, for the brightest illusions are often potent of the greatest harm. And here the psychologists themselves must stand most rebuked. For they have not always understood their own procedures, and have written as though they were treating of properly ethical problems when in fact they were seriously impugning the validity of ethical ideas.

Professor Flugel is a good example. His usurpation of the place of the moralist, in his recent *Man, Morals, and Society*, wears a most innocent appearance. For he gives no clear indication of the ethically barren land to which he proposes to lead us. One might often gather that his purpose was to treat of psychological matters subsidiary to distinctively ethical principles, and such expressions as "wrongdoing," "moral factors," "budding moral sentiments," "moral and social influences," "moral development," "a very genuine moral conflict," to select a few examples at random, appear in a disconcertingly normal shape in a context where they have suffered complete transformation. We are also disarmingly told at the start that, as psychology "is a positive, not a normative discipline," the author has "no concern with values as such,"* but this does not deter him from asserting later, in much completer consistency with his main theme, that "we must substitute a cognitive and psychological approach for an emotional and a moral one."† Of the easy assimilation of Scriptural sayings into a context most incompatible with their true purport Hobbes himself might well be proud. But the result of this is most seriously to darken counsel, and prejudice fair estimation of the author's claims. If, therefore, we find that the notion of guilt is reducible to psychological terms, there must be the fullest and most unambiguous repudiation of its claims to strictly ethical significance, and of all that is associated with it in ethics. To waver in that matter is fatal.

But are we reduced to such a pass? Is that involved in the concessions we have made to the psychologist? By no means. We may freely admit that there are such fears and desires as he describes, and that they take shape through the complicated processes which psycho-analysis reveals; formative influences may be deeply hidden from us in aspects of our personal history which we have mostly forgotten. But this is no bar to there being *in addition* guilt of a genuinely ethical kind incurred in certain ways. Most of what the psychologist avers may be allowed without

* Op. cit., p. 11.

† Op. cit., p. 255.

touching the properly ethical question. And, indeed, the significance of much that we are told about the repression of guilt and its projection on to others, and similar processes, may be vastly deepened and extended if such operations are performed, not merely on desires and fears but, also, on a true consciousness of guilt and its accompanying emotions. It must be a more serious matter, and presumably one where subtler and more determined resistance would be offered to therapeutic treatment, to suppress real guilt and drive remorse itself underground to erupt elsewhere, than to treat the pseudo-guilt of the psychologist in similar fashion. Furthermore, one wonders whether pseudo-guilt, with its roots in fear and retaliation, would have quite the tone that it has were it not for resemblance to genuine guilt. But without pressing this attack too deeply into the ranks of the analysts, and remaining mainly on the defensive, we may at least insist that there is nothing conclusive in their teaching about introjected emotions and subsequent projection, etc., as it bears on the problem of whether guilt, in the properly ethical sense, is real or illusory. This question could never be finally settled by recourse to the psychologist alone, for the essence of the claim to be determined is that there is something not to be directly encompassed by psychology. To leave the final decision here to the psychologist himself is to beg the question in completest fashion. Our doubts can only be finally resolved by reflection on what we do mean when we think of guilt. This reflection is conducted fairly only when the facts adduced by psychoanalysis are kept steadily before us. But, for my own part, I must admit that the presence of a plausible alternative account in psychological terms avails little to shake my conviction that guilt is a distinctively ethical attribute of conduct consisting in the peculiar appropriateness of blame to irreducible moral evil.

IV.

But if we are to think of guilt in this fashion there must be no illusions about the conditions which render it possible.

For it seems clear that no conduct is open to blame unless it is free, and it is a fair rejoinder to an accusation to declare that we "could not help" what we did. But what sort of freedom is this? It would be easy to reply if we could think of the moral law in some way analagous to the "law of the land," and, therefore, as having its sanction in the fact that we may be proceeded against or punished in some way, or if blame could be wholly described in psychological or naturalistic terms. For all we would need on that view would be the sort of avoidability which would give point to the reactions in question. Its nature has been very clearly presented by several writers, including Mr. Charles Stevenson in his recent *Language and Ethics*. As he notes, an action need only be avoidable, for the present purpose, in the sense that we could have acted differently had we wished. It is folly to be angry with a man for not giving us the moon, or to punish him. Our action could not mend the matter at all. But we may well be angry with someone who strikes us a blow, and take measures against him; for that may prevent a recurrence. It will also deter others. This is not all that the positivist has to say. He takes account also of angry exclamations as a relief to our feelings. But I do not wish to pursue this matter in any detail. It will suffice to express my full agreement with the main principle of the theories in question, provided it is true that guilt can be fully described in terms of a psychological reaction.

But we have contended that guilt is bound up with some inherently evil nature of conduct. And the freedom to act otherwise if we desire will not suffice here. This seems very clear with regard to the carrying of a purpose into effect. But not even this has always been allowed by moral philosophers. Without wishing to deny the uniqueness of ethical distinctions, they have often written as if the "freedom to do," in the sense of freedom to effect a purpose, met the requirements of ethics. But Sir David Ross, who gives much prominence to this meaning of freedom in his recent discussion of the "supposed intuition of freedom" admits very readily, that it has little significance for ethical theory.*

* Cf. A. E. Taylor, *Elements of Metaphysics*.

We blame those who have failed to carry out a wicked intention quite as much as those who succeed—if their determination to succeed is the same. The positivist could account for this by noting that a similar intention may be successful in future, and, therefore, the tendency needs to be curbed by punishment. But the common, less sophisticated, view would be that moral freedom must be freedom to choose or will, and not merely to effect what we choose, because the good or bad qualities which presuppose it are inherent qualities of the will itself. And this is equally fatal to the notion that moral freedom is the freedom to *intend* what we desire. For it seems very odd to hold that our conduct is open to blame, directly and irrespectively of deterrent effects, in regard to our intentions, but not in regard to the way our intentions are shaped by our motives. This latter, it seems, is Sir David Ross's view. He argues that we cannot summon up motives at will, any more than we can finally control the effects of action, and that, therefore, our duty is, not to act from certain motives, but simply to act or intend. And, as we have seen, he believes that it is intentions which we blame although moral worth depends mainly on motives. But, as I have insisted, blame is not to be divorced in this way from moral quality. And this shows how impossible it is to maintain that we are free to act, in the way required by responsibility, although we are not free in being moved to act. We seem, therefore, bound to conclude that the postulate of freedom required in ethics refers to some conduct which is free in a way in which neither motives, which, as I quite agree, cannot be summoned up at will, nor acts from certain motives are free. In other words, if we are to retain the notions of moral evil and guilt in a distinctively ethical sense, we have also to presuppose a power to act independently of our desires and characters—an absolute freedom of choice.

Failure to appreciate this is a prime source of confusion in recent ethics where philosophers have been seriously at cross purposes with themselves, especially in the controversy about the distinction between "the right act and the morality of the agent," by making extensive appeals to the

postulate of freedom without clear recognition of its meaning as a choice between genuinely open alternatives.

This I have discussed more fully in an earlier paper.* But there is one matter not mentioned there which I should like to note now. It seems plain that conscience is fallible, and that, therefore, we may do what is wrong with the highest intentions. We normally think that our moral worth does not suffer in such cases, and this is one of the main reasons for the distinction between the "right act" and the "morality of the agent." But it only holds if we think of the conditions of moral value as differing sharply from the conditions of some non-moral good like knowledge. Why, otherwise, should ignorance exonerate? But conduct which is the expression of motives and character has the same inevitability as appears in the course of our thought. And since this is the view of conduct usually adopted by those who have recently stressed the distinction between "the right act and the worth of the agent," their opponents, proceeding on the same assumption, seem on equally strong ground in urging that moral ignorance is itself a moral defect, or, more ambiguously that "we do not believe that everyone ought always to do what seems to him probably best."† There seems to be as much to be said on both sides as long as we seek to retain the idea of obligation within a deterministic view.

This brings us to one of the main ways in which it is sought to avoid admitting that moral distinctions require an open choice, namely by thinking of moral freedom as analogous to the freedom of the artist or the thinker. This is a theme in itself,† but very brief comment must suffice here.

The importance of the assimilation into our own nature of the forces which influence our thought, and which condition the attainment of values in other non-moral regards, may be readily admitted. The artist, the lover and the

* "Moral Freedom in Recent Ethics," *Proceedings of the Aristotelian Society* 1946-7.

† John Laird, "On Doing One's Best," *Philosophy*, Jan., 1931.

saint are, in one sense, supremely free. They are also determined, for they could not be what they are without their environment. But their environment is made a part of themselves. So also, the better our thought the more truly is it our own. But good thinking also conforms to the laws of thought, and the more intelligent we are, the more are we bound to think in a certain way. In these activities, therefore, freedom and necessity meet. And this has been thought to bear on the problem of moral freedom. But this is very mistaken. For the necessity of duty is the necessity of a command, it is not identical with freedom but presupposes it ; moral freedom is thus not a freedom which can be equated with attainment as is the freedom of the artist or the thinker, but rather a freedom to go one way or the other, and it is not a whit affected when we choose the bad. This is what is brought out most clearly by reflecting on the guilt which immorality involves, and the only way in which the celebrated theory of freedom as "self-determination," namely as assimilation into our character of the forces which shape it, could meet the requirements of ethics would be if we surrendered the ideas of guilt and merit, and of moral evil as the individual's rebellion in the face of a "categorical imperative." Our ethics would thus approximate closely to the Greek view. For the Greeks had little consciousness of guilt—so far as their main philosophical thinking went—and they thought of moral goodness in ways very similar to the general cultivation of "a gift of nature." The moral end was some fulfilment of ourselves, and men went wrong because they did not understand what they really wanted ; virtue was knowledge. There was no sharp distinction between moral and non-moral good. But this will not content the advocates of "self-determination." As in Ross's case, they are usually most anxious to retain the ideas of obligation and blame which figure so little in Greek ethics. But to do so they must postulate a freedom which is other than the freedom also involved in art or knowledge where blame seems plainly out of place—just because there is not the requisite freedom. This is a freedom of choice, a freedom to act or not to act,

in a certain way, and if moral blame is not to be reduced to naturalistic terms, this must, furthermore, mean an absolute freedom of choice between open alternatives.

To accept such a choice is not to imply that any action may be expected from any man at any time ; such a position would be as absurd as any position can be. All that is required is that there should be some occasions on which the flow of our conduct in accordance with character is arrested by a contrary claim of duty. This, in turn, also presents many difficulties, of which the greatest, to my mind, is that of assuring ourselves that an action which seems to involve a free effort of will, or which is not "in the line of least resistance," does not in fact issue from some element in my character which could be revealed by more careful introspection. But for reasons of space I cannot embark here on a general discussion of indeterminism. I will content myself with the insistence that we can only retain the ideas of obligation and guilt, as properly ethical ideas, if we can also believe in actions which could have been other than they were although everything else in the universe had remained the same. I content myself with this because there is another aspect of our problem to which I should like to turn before I close.

V.

As we have seen, it is when we think of moral evil as guilty or blameworthy conduct that we have also to regard it as wilful violation of law, as disobedience, and, thus, as involving absolute freedom of choice. But, very strangely, it is the persons who seem most assured that guilt is real, and who would most stoutly resist the attempt to reduce it to psychological terms, or to dismiss it as an illusion or some matter calling for mere therapeutic treatment—it is these, religious thinkers mostly and concerning themselves much more than the moralist with the problem of guilt, who also, as a rule, seem most emphatic in repudiating freedom. Even when they pay lip-service to freedom of choice, they belie it in their deepest and most distinctive convictions. This I shall call "the paradox of guilt." It is very

deep and persistent, and it presents itself in fiction and general literature as well as in religious thought and reports of religious experience. Where there is the greatest assurance of guilt, there also we often find least concern for the individual and his freedom, and a consciousness of being in the grip of destiny. And, rightly understood, I believe that this has the greatest significance.

Some matters preliminary to the main explanation of this paradox are these.

There is a tendency for religious thinking to be more conservative than any other part of our thought. This is so for several reasons. But the most important reason, perhaps, is the fact that early religious experience carries a certain superficial authenticity which is absent from more mature religious life. This is because man is closer to Nature and more at her mercy. His life is less protected, and the world about him has not been so subdued to his thought as in more enlightened ages. He wages a more constant battle with reality, and there is thus a greater sense of awe and mystery, of the pressure upon him of alien, unrelenting powers and of a world not made amenable to his own will. The problem of subsequent ages, and above all in religion, is to recover this sense of an alien reality, of what is not ourselves or dissolvable into the categories of our own thought, at the level of new attainments and the conquests of the mind, to bend back our powers into contact with things, and thus be more truly and deeply at one with reality than was possible in the cruder immediacy of primitive life. It is this that we seek in the present crisis of Western Society, but it is not easy of attainment. And the starker, if unedified, realism of remote times confers on their religious life in particular a genuineness which may be equally present in more sophisticated experiences, but is rarely so obvious. And, therefore, the religious ideas which have taken shape in earlier and darker periods of society are apt to carry with them afterwards a peculiar claim to be the most essential and authentic versions of religious truth. But primitive society pays little heed to the individual ; it identifies him, in his moral activities, with his

community, with his family or his tribe ; and the inexorable rule of custom operates with little regard for such niceties as the distinction between the purposes and the effects of actions. Accidental wrong initiates as surely as the most open maliciousness the savage alternation of retribution and retaliation. It is thus not surprising that so much in our religious thought, hailing from this source and also made more inflexible by their origin, should lag behind more enlightened views about freedom and accountability.

In reinforcement of this, it is worthy of note here that the assertion of guilt incurred in other ways than by avoidable human action, is bolder in times of confusion, like the present, when there is a recrudescence of the primitive and pagan attitudes which lurk beneath the surface of our civilization. This seems to point to a deep kinship, and not to a mere accident of the times, as might be well brought out from a close study of the course of religion in Europe of late.

But, further, there is a very important way in which it is natural for religion to fill us with a sense of unworthiness, to make us "as dust," "as nothing" ; the fleeting and unsubstantial aspect of existence is contrasted with the permanent realities of religion. But this, as has often been remarked by writers on "the sublime" and "the holy," is not moral unworthiness, but, rather, a general sense of unworthiness induced by the confrontation of the individual with the absolute perfection which he senses in his religion. God is all, man is nothing, his "days are as grass, as a flower of the field so he withereth." But this general sense of the dependent and limited character of finite being, and sometimes of utter hollowness, is easily confused with a sense of moral unworthiness, especially in those indiscriminating formative periods in which the more permanent idiom of religious utterance is coined. Moreover, the sense of the distance of the finite creature from the absolute perfection of religion does sharpen a man's ethical consciousness also and make him more vividly aware of the evil nature of the sinful acts that he has performed. And this makes it easier for the feeling of nothingness with which we are overwhelmed

in religion—but that is only one aspect of the matter—to present itself also as an all-pervasive consciousness of sin.

With the greater progress of religion other factors enter in. This in particular. As religion becomes more completely moralized, and takes its place as the main source of refinement in ethics, the individual often finds himself confronted with exacting ideals which he is not able to embody to the full in the circumstances of his time and his society. The visionary must carry his neighbour with him, and this means some lowering of standards, much as the socialist to-day has to belie his principles about the ownership of property until he can persuade his society to accept them as a rule of life for the community as a whole. Some compromise is unavoidable, and the structure of society itself, moreover, impedes moral effort in some ways—even if it helps it in others. Hence, to borrow the title of a notable book, we have “moral man and immoral society.” But this adjustment to a more common denominator rests very uneasily on a sensitive conscience, and the more sensitive are also often the most prone to measure themselves, not by what is attainable in the special situation in which they are placed, but by the ideals which glow more brightly ahead, by “impossible ideals” and “patterns laid up in heaven.” This also leads to a sense of sinfulness and guilt not resting on individual choice, a matter that is no less distressing to the individual because it has such little foundation. It is significant that the idea of a tainted and fallen nature became most distinctive of Christian thought when the church was passing through a similar crisis. The early disciples of Christ obeyed his revolutionary injunctions—much more revolutionary than we usually acknowledge—with a great deal of consistency. But by the third century the church, for good or ill (but more for ill, I think), had immersed itself more completely in the world, and had to regulate its life by a “relative” rather than an “absolute” law. It was then, in the teaching of St. Augustine and his successors, that the notion of a tainted nature, of unavoidable sin, and the hypostatizing of the universal nature of man in the doctrines of the Fall and of universal guilt acquired

their ascendancy in Christian theology. They have had most prominence subsequently in times of similar crises, in the nationalist upheavals of the 15th century and in great modern wars when Christian ideals are most obviously strained.

This is not without its sinister aspect. For the idea of unavoidable sin is easily exploited. It presents an attractive way of turning the edge of inconvenient standards. The ideals remain in our midst but go unregarded. And Christians have in fact often, singly or collectively, sometimes with more, sometimes with less, deliberation sheltered from the rigorous exactions of their Christian allegiance behind the doctrine of the radical corruption of man and the domination of his life by impersonal evil forces. Something of the glow of the ideals is retained, or, at the level (rare, I believe) of crude expediency, their prestige, without too inconvenient an exaction of obedience. Such accommodation accounts for more in the history and thought of Christianity than is commonly realized. Along with this there go a delight in denunciation, in exploitation, without great emotional and spiritual cost, of the dramatic possibilities of a tragic situation, and, by a subtle inversion of pride, in vilification of self. Of these there is much evidence at present. The result is twofold. On the one hand, we have an alienation of religion, and often of ethics, from the business of living, a serene irresponsibility ; ideals become, not rules of practice, but standards by which we are judged and condemned, salvation in turn being mediated in the cosmic drama of religion independently of our will. On the other hand, we have a sullen despair and pessimism, an enervating sense of the futility of all human endeavour. Both attitudes are very marked to-day, and they call for the closest and boldest intervention of the moral philosopher. But I mention these matters here mainly because of the lease of life which is lent in these ways also to the paradox of guilt.

But we have yet to offer the main explanation of this paradox and the reason for its significance. This presents itself when we consider the function of morality in the

economy of life as a whole. Mention has already been made of the need to discover reality anew, and at a new power. And it is here that morality serves us so well, for its discipline is unlike any other, not excluding that of suffering and pain. For, although reality impinges upon us in pain with a peculiar insistency, this is because in a measure it has become ourselves. It is subjectively mediated, as also in perception where the pressure of reality upon us is rudest. Even in art where reality *is* made anew, where it is most individualized and expressive, and when the day-to-day rigidity of things is broken, when they stir and become articulate, even there our minds are only turned outwards to reality, as plants to the light, because of its fascination. Art could do nothing with us if it had no appeal. Neither would there be forgetfulness of self in personal attachments were not our affections involved. But morality knows nothing of this, there is no subtle transformation here, no fusion of reality with self. Morality provides no lodgment for the real in our own affections. And this is why the sense of reality is sustained by the moral life, and kept from distortion. Duty does not beguile, it commands ; it can have no lure. What is not self is starkly before us, insistent, not to be denied ; we are left with it as in an utter void. If we prevail there is a tremendous sense of exaltation, a heightening of all our powers ; the sheer, rugged, external shape of reality recedes ; it is serene again. If we fail there is a diminution of our powers, a desolation, a kind of doom or death which is the penalty of sin. From this we may escape in part by sharp awareness of guilt and confession, and by the thrusting of the evil nature of surrender, to self deep into our consciousness in condemnation and remorse* ; and by grace, even more, the inward flow of reality begins again. What we are told of this in religion is often repellent, because the idiom and structure of religious thought hardened so much at the time when it was still mingled with a barbaric ethic. But the alienation of

* It is by confusion with this that the notion of retributive punishment acquires some plausibility.

self from reality in moral failure is often so overwhelming, and most of all for religious minds, that we come to regard that failure, not as the cause, but as the effect, of some general impotence, of a doom in which we are caught up, of evil forces warring in all our members. Hence it is that the deepest consciousness of moral evil, and the sharpest recognition of it as guilt, often carries with it the belief so contradictory of all morality, that our guilt is itself the working out of some radical weakness in our own nature.

This, and much else in the relations of morality and religion, and of both to all forms of experience, we shall understand much better when religious thought is able to mount to the level of our other attainments. We shall then have the clue to the crisis of which so much is heard to-day, but which is not quite so peculiar to our time as is thought.

II. By J. W. HARVEY.

IN the main, I confine myself to two questions : the meaning of " guilt " as an ethical idea, and of the " freedom " which this guilt presupposes.

I.

Guilt, Mr. Lewis would have us recognize, is a large and complex but " first and foremost " an ethical problem. I am not quite clear whether he regards the complexity as solely one of ramification or rather one of equivocation. In point of fact, it is both ; that is, we have to do both with a problem which, as its bearing upon this or that field of experience comes to be considered, generates other consequential problems because the fact with which it is concerned has manifold facets ; and also a single term that stands ambiguously for a variety of meanings. It may be well to expose these ambiguities at the outset. At any rate, I should prefer to say, not that " guilt " is " first and foremost an ethical problem," but that along with other ways in which the term is and may permissibly be used, in jurisprudence, psychology and theology, to say nothing of " common parlance," there is an important and distinct *ethical* use of it, pointing to moral facts which cannot be adequately appreciated by those who confine the term to its psychological or forensic meanings. (I shall urge that the theological or religious stands in a much more intimate relation to the ethical meaning.) I do not think that either Mr. Lewis or Mr. Paul avoid the confusion due to these ambiguous usages. The former, for instance, in spite of his definition of ethical guilt, to which I turn presently, allows himself to speak of the contrast between " pseudo-guilt " and " real " guilt, and the " suppression " of the latter as though the term meant in each case an emotional *state of mind*, the difference being in whether there is or is not a true appraisalment of the facts prompting the state of mind. Does the " suppression " of " real " guilt mean, to alter our judgment (and its emotional concomitant) *about* the character of our conduct or to alter the character

itself? And Mr. Paul seems to tolerate the same equivocation when he asks in what the psychotherapist and the religious therapist differ, seeing that both "treat guilt as a matter calling for *therapeutic* practice." This is so, surely. But the implied identity of the subject is only to be found if the term "guilt" means for religion *and* psychology no more than the subjective attitude of an agent to his own wrong actions.

Let me then briefly attempt to disentangle several meanings. (1) Of the *forensic* meaning I think we may say (a) that the term is applied primarily to *persons* and only secondarily and derivatively to acts or conduct, that conduct having "guilt" or being designated as "guilty" which is *evidence* of guilt in the agent or/and *brings about* guilt in him. (b) It indicates a complex relation between an agent of a certain quality (sanity)* and an act of a certain legal quality (criminality), involving causality, intention and wittingness. It is concerned with relations between elements in an ascertainable objective factual situation and not with an agent's opinion about or feeling regarding that situation. A guilty person *is* guilty whether or no he feels so. (2) For psychology the typical meaning of "guilt" would appear to be a state of mind, *i.e.*, self-blame and the accompanying characteristic emotion, a feeling of stigma. The psychologist is not as such concerned with the question how far the blame has adequate ground or justification. But he is certainly not committed to denying that it may be well justified, *i.e.* that the person may be "guilty" in something like a widening of the forensic sense, or that there may be another sense in which the term may be used for such cases as involve "real" wrong-doing. I agree with Mr. Paul that psychologists do not quite deserve the strictures of Mr. Lewis. The passage cited from Dr. Karen Stephen could certainly be paralleled from the writings of other psychologists, Freudian and non-Freudian. (For that

* "Guilty but insane," though retained as a convenient legal fiction, is, in fact, a paradox, due to Queen Victoria's insistence that her demented would-be assassin must be accounted guilty. An imbecile cannot really be guilty of a crime, nor an animal.

matter, I don't think Mr. Lewis is fair in his interpretation of the passage quoted from Flugel.) From the fact that make-believe "guilt"-situations (giving rise to the guilt-emotions in which the psychologist is specially interested) are a very common fact of psychopathology, it no more follows that there are no "genuine" guilt-situations than it follows, from the fact that we constantly "rationalize" our behaviour to make it look reasonable, that no behaviour is or can be *really* reasonable. If anything, the reverse; the counterfeit implies an authentic legal currency.

For the psychologist, then, "guilt" is a term qualifying a state of mind, and only secondarily the person whose mind is so characterized. For the psychologist it is therefore natural to speak of *feeling* rather than of *being* guilty (just as for the hedonist it is more natural to speak of feeling happy than of being happy.)

(3) What now is the ethical meaning? Mr. Lewis defines it (end of III) as "a distinctively ethical attribute of conduct consisting in the peculiar appropriateness of blame to irreducible moral evil," and the kind of moral evil or wrong action he has in mind is deliberate disobedience to an acknowledged duty, a wilful violation of moral law. There are two points here which I cannot accept. First, I believe that we are bound to misinterpret the matter if we make ethical guilt an attribute of conduct rather than of persons. Here as it seems to me ethics is at one with the "forensic" usage and differs from the psychological usage. (As before, of course, the term may be applied *derivatively* to the act or piece of conduct.) What I think this indicates is that the ethical "problem of guilt" is misconceived by those who take as the central fact, as, so to speak, the ethical unit, the act or even the state of mind and not the person who acts and has the state of mind. It is not without significance that those ethical thinkers who have least interest in the notion of guilt are those who, like the Benthamite hedonists, have little to say about persons in comparison with the emphasis they put upon aggregates of feelings or of serial experiences.

My second cavil concerns the close union of moral guilt with blameworthiness. Mr. Lewis is I am sure absolutely right in rejecting the naturalist view that guilt is to be *defined in terms of* blame, still less of punishment. I gather that it presupposes that we know what we mean by "irreducible" or "deep and ultimate" wrong-doing independently; the notion "guilt" *adds* to this the idea that such conduct is gravely blameworthy. Blameworthiness becomes then the differentia distinguishing such wrong conduct as is guilty from that which is not. *Blameworthiness* implies, of course, a standard: it is not the same as "blamedness" and therefore involves no surrender to naturalism. All the same it brings the idea of blame and of guilt too closely together. For if it were correct it would be a mere tautology to say: This is guilty conduct and *therefore* the person who acted thus rightly incurs blame. Or rather it would only *not* be a tautology if the "therefore" were simply a verbal elucidation of a strange term. But I do not think the above statement *is* tautological, and therefore I cannot quite accept Mr. Lewis's definition of ethical guilt. But something of the sort is almost unavoidable so long as the term is taken to be an attribute of conduct or acts rather than of persons.

"Blameworthiness" is an idea closely bound up with that of guilt, certainly; all guilty acts (or, better, persons) are blameworthy. But the relation of the two terms seems to be rather like that between the notion of a political State and that of coercive regulation. It will hardly do to rely wholly on the notion of coercion to define that of a State, though no state can deserve the name unless it comprises governmental coercive institutions.

But if this is not the meaning of the idea, what other is there other than merely emotive meaning?

But if that were all, there could be no question of claiming the sort of "reality" for guilt which Mr. Lewis does not hesitate to affirm, and which I am sure he is right in affirming. And I suggest that the meaning we need can be found only in some objective implication of the breach of duty or rebellion against the moral imperative

which is stigmatized as "guilty." And this I take to be the self-alienation from the moral order which the guilty person has brought about. His action, if the term "guilt" is appropriate to it, is not only disobedience to a law but disloyalty to a world, the moral world, by which I mean, I suppose, something like Kant's Kingdom of Ends or Royce's "Beloved Society." Not all breaches of the moral law have this character ; where there is doubt about the matter, we hesitate to speak of guilt. (This indeed provides the themes for great tragedy. Was Orestes truly guilty ? He had committed a frightful crime, but in obedience to a supposed duty. A rebel against a basic law, was he also an outlaw from the moral world as a whole ?)

The question of the "reality" of ethical guilt will then depend upon whether such phrases as I have used, indicating a solidarity of moral wills or persons, stand for anything "real," or are mere figments embellishing a vacuum. Rejecting them, we shall probably not take the idea of guilt very seriously, if indeed we tolerate it at all. But to me the idea cannot but be taken seriously ; I accept on that point to the full Mr. Lewis's contention.

(4) But what seems to appear in this connection is that in so far as the conception of a moral community-fabric, in part actualized in part only adumbrated, is accepted as meaningful, we are almost forced to pass from the merely ethical to the religious standpoint. For what is the theologico-religious meaning of "guilt" ? As before, surely, it is an attribute of persons primarily, not of acts, but of persons seen in a wider than a merely human context, as standing in relation to a moral order that includes somehow, or is based upon, a superhuman and supernatural reality. (I am merely noting what I take the religious standpoint to mean, not trying to elucidate or commend it). It does not perhaps make much difference whether the religious thinker stretches the ethical terms to include this wider reference, as St. Augustine does when he speaks of the sin of impiety as injustice towards God ; or professes to use such terms as "guilt" in a sense transcending the ethical. In any case he seems to be asserting that guilt

is incurred when a human agent commits an act which, so far as it goes, alienates him from the divine-human moral order, as some kinds of law-breaking indicate a self-estrangement of the citizen from the political order of the State.

Thus the religious point of view, if it has any solid basis, will reinforce the ethical interpretation of "guilt" by putting it in a wider context and relating it to a more comprehensive reality.

There are two points in which I think the conception of guilt as emphasizing the social disloyalty of an act of moral disobedience avoids difficulties. The first concerns the sin, traditionally accounted a sin involving guilt in a peculiar degree, the sin of pride. If all the stress is laid, as Mr. Lewis appears to lay it, upon conscious rebellion against the moral imperative as a precondition of guilt, then it is difficult to see why such guilt should attach to pride. The morally complacent man, the Pharisee, is clearly not in rebellion against any recognised duty. He is pleased with himself precisely because he has, as he thinks, successfully met all the claims of duty. But on the view indicated above, he will incur guilt in consequence of the self-centredness which shuts out from his view the complex fabric of the moral community to which he belongs, and in which his place is inevitably a subordinate one. There are, after all, other ways of being a bad and disloyal citizen than by starting a rebellion against the accredited government; there is the self-estrangement of the social parasite.

The second advantage of the suggested interpretation concerns the relevance of punishment, but I have no space to do more than touch on this topic. Mr. Lewis' way of stating the matter comes, after all, I think, too near making "punishworthiness" the differentia of "guilty" wrongdoing, for "blame," in one aspect, is a sort of punishment. But if the point is rather the character of the agent as inflicting a lesion upon the "body ethical," the "moral community," the "Zusammengehörigkeit" of persons, then punishment falls into its place as one aspect of the

process of undoing the injury and restoring the lesion, a process which will assuredly normally have its drastic "retributive" side (bringing the offence home to the guilty person) as well as its rehabilitative side. Only so can we avoid the two opposite sentimentalisms of condoning the offence by mere therapeutic treatment of the offender and dooming the offender by sheer vindictive condemnation of the offence.

Nor need we be puzzled at the fact that the opposite idea of "guilt," viz., "merit," is no true correlate and lacks its sharp ethical pungency. For "guilt" is the abnormal, its opposite the normal attitude. It is normal, that is to say, to "maintain the fabric of the world" (the moral world) and not repudiate it. It is rather as though we found it a matter of surprise that while divorces and murders have "news-value" there is none in the fact that Mr. and Mrs. Jones are still happily married and that Mr. Brown is still alive. But there is also this further consideration, which points to a "paradox of guilt" other than that which Mr. Lewis calls by that name. It is the fact that while clear recognition of the reality of his guilt by the guilty person is a necessary precondition for overcoming it (*i.e.*, for contributing to the healing of the moral lesion it involves), the clear recognition of his merit by the virtuous person seems inevitably to tarnish it and will lead to the "guilt" of moral self-complacency already alluded to. It is this taint which hangs over so much of Greco-Roman ethics. The virtuous man of Aristotle or the Stoics is almost bound to be something of a prig, and one applies to him what was (unfairly, I think) said of Marcus Aurelius, "a great and good man; and he knew it." The problem in this paradox: how is it possible for a really good man not to be aware of his own merit? Yet how can he be aware of it without incurring the guilt of presumptuousness and pride?—is ignored in Greek ethics, but finds poignant expression in another quarter. It is the problem of the Book of Job. "My righteousness I hold fast and will not let it go: my heart shall not reproach me so long as I live." Yet also: "If I justify myself mine

own mouth shall condemn me ; if I say, I am perfect, it shall also prove me perverse." Clearly we are on the wrong tack in contrasting "guilt" and "merit" like the debit and credit side of a ledger account.

It may be said that the phrases here used to denote the implication of moral guilt are merely pretentious periphrases for the fact that *all* morally wrong action is *anti-social* action. I do not think so ; or rather, the whole point is, whether "anti-social" itself means "to the several disadvantages of other people" or "a blow at the moral solidarity that constitutes the fabric of social good." Is there or is there not anything real denoted by such a phrase? I believe there is ; but, of course, if there is, it will not reveal or verify itself to any observational technique such as Mr. Paul seems to hanker after in these matters.

There is not space to touch on the bearing of this upon the theme mentioned, but not discussed, by Mr. Lewis, that of collective guilt, of which "war-guilt" is the obvious contemporary instance. I believe that if "guilt" be taken as a function, so to speak, of *acts* there are difficulties in finding a satisfactory interpretation for the conception of corporate guilt ; we are driven to distribute the guilt among all the members of the group or collectivity in question, so that every one of them (as an individual) "shares the blame." In truth, *every* German *need* no more share the blame for "Germany's" crimes than he shares the credit for Germany's musical achievements. None the less, just as he may in a real sense take pride in the latter, so he should take shame in the former, *i.e.*, be repudiatingly conscious that he has become part of an enclave in the moral world which collectively has become estranged from the solidarity of ethical achievement.

II.

That "guilt" in the ethical sense presupposes responsibility and that this in turn cannot be interpreted as

"fitness to be punished," but implies freedom to act or not to act, seems quite undeniable. I think Mr. Lewis is too severe on this point in attributing the opposite view to idealist thinkers.

But I am by no means so clear that he is right in claiming that if deliberate conscious wrong-doing is to be possible, a drastic form of indeterminism has to be accepted. "Self-determination" of the type familiar in idealist ethics does not, he holds, give us what we need. It would seem that on this point he has modified his view since his paper on *Moral Freedom in Recent Ethics* (Aristotelian Society, Nov., 1946). There we gather that while sympathetic to the Indeterminist point of view on some counts, he is daunted by some of its implications, and on the whole hesitates to recommend it. But "if we find that we have to reject the libertarian view, then we must also be ready to sacrifice much that is usually considered important in ethics, including the common conception of guilt and responsibility." This will involve "drastic changes in our ethical thought," but much will remain that is of value. And the task of revision must not be shirked, for that will leave the field to types of thought (naturalistic and psychological) which will be likely to prune away much that could and should be saved. But in the present discussion there seems no such hesitation to accept a drastic Indeterminism, in fact, we seem to be given no alternative. Because (as the former paper puts it) "guilt, like the Categorical Imperative, simply cannot be accommodated at all within a deterministic scheme"; because the familiar Idealist "interpretation" of human freedom as self-determination is a virtual surrender to determinism, another and more fundamental freedom is (he thinks) to be affirmed, if we are to accept guilt and obligation as genuine—as we cannot but do.

I believe that the conception of self-determining freedom, at any rate as sometimes expounded by Idealists (including Kant) is justly open to some criticism. It is a pity that in ethical, as even more in political discussions, there appears to be an irresistible attraction about the

word "freedom," inducing those who use it to load it with positive meanings it ought not to be made to bear. They speak of *freedom* when in fact they mean *power*, a power increasing as the self incorporates more and more experience in effective unity. None the less, I suspect that Mr. Lewis is not quite fair to the doctrine he criticises.

He contrasts the "superior way of being determined" (i.e., "self-determination") illustrated by the activity of the thinker or artist in which freedom of creation and conformity to principle, logical or aesthetic, are mutually involved or even two sides of the same fact, with the freedom to do or not to do which is *presupposed* in moral action, the "freedom of open possibilities" which is the same for the bad as for the good will. Now it is true enough that the analogy between the freely thinking mind enabled by its obedience to rational principle to resist the solicitations of irrelevant association and the free choice manifested in a "moral situation" can be misleadingly presented in a way that obscures the primacy of the latter. For to think or not to think, to attempt artistic creation or not to do so are after all possible *cases* of a moral situation, as the challenge of every type of experience may be. The analogy is not therefore between co-ordinate facts, but between a superordinate and a subordinate. But I think Mr. Lewis mis-states the analogy because he is contrasting "open" freedom of moral choice *before the choice has been made* with self-determined "freedom" in thought (or in art) *after the choice has been made*, or when it is being implemented. To invent a phrase, it is as though he contrasts a "*libertas liberans*" in the one case with a "*libertas liberata*" in the other. Naturally, the latter does not seem to give *pre-conditional* but only *resultant* freedom, freedom as an effective *achievement*, "a part of the goodness of the action." But, surely, if we adopt the same point of view in both cases, we must recognise in the experience of the thinker or the artist the analogue of the "freedom to do or not to do" upon which Mr. Lewis insists so strongly. Is it true that the poet for instance *in the actual creative process* of making his poem is conscious that he "must" use this or that

expression? * *After* he has chosen, we, and perhaps he himself, look upon his words as inevitable. *Before* he has wrought, the artist is conscious of alternatives and of the freedom to choose this or that. So, too, the thinker. It is only in the most abstract and formal thought-process that there is the sort of *a priori* constraint which Mr. Lewis seems to find characteristic of *all* thought-processes, even that of the creative artist. More often where a mind is "freely" mastering its material and developing its thinking effectively and rationally, there is no such inevitability or constraint experienced, though there too *after* the work is done we can recognise that it did in fact conform to the controls of rational principle.

It seems to me, that is to say, that what is of value in Mr. Lewis' contention regarding the sort of freedom that must be presupposed if moral choices are to be genuine is presupposed in self-determination, though, incidentally, to the development of the self rather than in the more drastic and rigorous form which Mr. Lewis prefers and which does not seem to me free from difficulty.

It is perhaps true that moral conflict has been presented too much (as in Plato's famous exposition) as rebellion *within* the self—symptomatic of its failure in progressive unification—rather than as rebellion *of* the self, when, in fact, it may be both. The result of such misrepresentation is that the "guilt" incurred is taken more as a function of a part of the self—perhaps even a peripheral part—than as characterising the self as such. Mr. Lewis' stress upon the freedom of open choice is valuable as a corrective to a tendency making for self-exculpation.

* C.f. "The poet does not know till he has said it either what he wants to say or how he shall say it. The imaginative experience supposed to be in his mind does not exist there. What does exist is the subject, which detains him and fixes his thoughts and images, and passions, and gives his excitement a colour and direction which would be different with a different subject."—S. Alexander, *Beauty and Other Forms of Value*, p. 59.

III.—By G. A. PAUL.

1.—*Is it specially difficult to give a naturalistic account of the meaning of "guilty" ?*

Mr. Lewis thinks it is much more difficult than giving a naturalistic account of the meaning of "good." I am not convinced by the reasons he gives for this, and from his own definition of "guilty" we can see that it is not so. He says that when we say that a man is guilty we mean that he has done something morally *bad* for which it is morally *fitting* that he should be blamed and feel remorse. So it is only if we think it much more difficult with "bad" or "fitting" than with "good" that we shall think it much more difficult with "guilty" than with "good."

Whether it is possible at all to give a naturalistic account of the meaning of any of these terms at all—"good," "bad," "fitting," "guilty"—is a question we shall not discuss today, for we shall follow Mr. Lewis in wishing to avoid for the present "the general problem of naturalistic ethics."

2.—*Psychoanalysis : Does it aid the naturalist ?*

After emphasising how stubborn is the ordinary man's belief that guilt cannot be fully described in psychological terms, Mr. Lewis says, "It may be urged that it is just here that psychology, and especially *recent* psychology, comes to the aid" of the naturalist 181.7. Its aid consists in showing that "there are many hidden anxieties which *create the impression that there is some distinctive ethical guilt*, when in fact there is nothing but some emotional reaction. On this basis some thinkers, and especially followers of Freud, have maintained that guilt is to be conceived, in the last resort, as a 'need for punishment.'"

The aid is of two kinds :—

(1) A new naturalistic definition of "guilt," namely, need for punishment.

(2) A new explanation of how ordinary people come to believe that guilt is non-natural and distinctively ethical whereas in fact it is natural and psychological—"there are many hidden anxieties which create this impression."

I find it difficult to come to grips with Mr. Lewis here, for he does not support what he says from the writings of any Freudians. I hope that in time he will do so ; for the present I shall content myself with a short discussion of what he says about (1), and with giving some documentary evidence that not all Freudians would claim to provide either (1) or (2).

2.1 : *Does "guilt" mean need for punishment ?*

2.11 : "This question," says Mr. Lewis, "could never be finally settled by recourse to the psychologist alone, *for the essence of the claim to be determined is that there is something not to be directly encompassed by psychology*" 185.6. I agree, it cannot be settled by the psychologist, but not for the reason Mr. Lewis gives. He implies that guilt is directly encompassed by moral philosophy, and that therefore it is for the philosopher to settle the question. Similarly, I suppose, matter is directly encompassed by physics, and therefore it is for the physicist to settle the corresponding question about matter ; and the self is directly encompassed by psychology, so it is for the psychologist to settle the corresponding question about the self. But we do not in fact believe this in the case of matter or of the self ; and even if it were a question of deciding between two *naturalistic* definitions of "guilt," or even between two *naturalistic* definitions in *psychological* terms, we should still not think it a question for the psychologist to settle. Why not ? We need only look at the form of the question, in Mr. Lewis's own words, "what do *we* mean when *we* think of guilt ?" 185.7. It is not "what does *the psychologist* mean when *the psychologist* thinks of guilt ?" Now it is not that about which the question is asked (the self, matter, guilt), but the kind of question asked about it that makes it one for the philosopher, namely, "What do we mean when we think of *x* ?" It is philosophers who have put in the work on questions of this form.

So Mr. Lewis's point is :—Psychoanalysts have proposed a new naturalistic definition of "guilt." Do not be brow-beaten by their grand language and professional status. Judge their attempt by just the same criteria as you would any other definition.

2.12 : When the new naturalistic definition, need for punishment, is judged in this way it is quickly seen to be in fact no help to the naturalist. The trouble is not that it is naturalistic but that it is not even a plausible attempt at a naturalistic definition, and it would surprise me to find a naturalist who, having tried his hand at defining "guilt," had not done better before ever the Freudian came to "help" him. Compare "needing punishment" with Mr. Lewis's definition, "requiring blame and condemnation" 178.4. The difference between "punishment" and "blame and condemnation" is comparatively unimportant here ; the main point is to compare "needing" and "requiring." These two words are often used to mean the same, and in fact we would have understood Mr. Lewis very well if he had spoken of "requiring punishment" and "needing blame and condemnation." Mr. Lewis explains that "He *needs* punishment" means "Unpunished he *feels* a doom hanging over him, and *will experience relief only when* punishment has been administered" 181.9, and that "He *requires* punishment" means "Punishment would be *appropriate* to his conduct" 178.5. Probably "needing punishment" can pass for a definition of "guilty" only because "needing"* can be taken to stand not only for "*feeling* a doom, etc.," but for "being *appropriately* dealt with by."† But once it is decided that "needs" is to mean "*feels* a doom, etc.," the definition will not pass for a moment, for it is so obvious that often when we call a person guilty, so far from meaning this, we are well aware that the very last thing that will give him relief will be to be blamed, condemned, or punished ; and what we are saying of him is, of course, as

* cf. the possible ambiguity of "demands recompense."

† "He needs a good talking to."

Mr. Lewis says, that *whether it will give him satisfaction or not he deserves something of that kind.*

2.2 : *Do psychoanalysts in fact claim that "hidden anxieties create the [false] impression that there is some distinctive ethical guilt" ?*

I cannot answer this question in general. All I do here, and it is very little, is to quote from Dr. Karin Stephen,* a Freudian analyst, showing that she at least does not make the claim, but does say something that might be mistaken for it, namely, that *in every psychogenic illness* hidden anxieties create the impression that the patient *has some guilt which in fact he does not have.*

She writes, "*Neurotic guilt, a painful emotion which in some form is always present in psychogenic illness, thus results from the presence in the unconscious of repressed sexualised hostility. The neurotic behaves as if he had a bad conscience . . . Very often the patient is conscious of these feelings of guilt and worthlessness, and such feelings may weigh crushingly even on people whose lives are apparently blameless or even estimable*" 194.9. And "*it does not even seem necessary that their temper should have actually done serious damage : it is enough that they intended to do it, and in their omnipotent fantasy they will seize upon outside events for which they were not really responsible and claim the guilt for these*" 196.9. "'I did injure and rob them, and I can never do enough to atone for it.' Miss M.'s belief that she *had actually killed her rival and hurt her mother* sprang, ultimately, from her strong wish that this should in fact be true. Throughout her childhood she was burdened by the need to atone for this guilty success. To recognise that *she was not to blame* meant giving up her secret triumph, and she was unwilling to do this" 202.6.

Dr. Stephen makes clear that her patient has not "actually done serious damage," has not "actually killed her rival," "is not to blame" for any such thing, is "not really responsible for" an outside event and so cannot rightly "claim the guilt for it." This way of speaking

* Psychoanalysis and Medicine : a Study of the Wish to fall Ill (Cambridge, 1933).

implies that Dr. Stephen is aware that a person may do serious damage, be to blame for it, recognise that she is to blame, and justifiably claim the guilt for it.

What Dr. Stephen does say here is that *in every case of psychogenic illness* there is something for which the sufferer wrongly claims the guilt. (I should not be surprised if she were prepared to suggest that *everyone* has something for which he *wrongly* claims the guilt.) Now this way of speaking suggests that of course there are also things for which people do *rightly* claim the guilt, that there are things for which people *really are to blame*.

Mr. Lewis says, "It must be a more serious matter to suppress *real guilt* and drive remorse itself underground to erupt elsewhere, than to treat the *pseudo-guilt* of the psychologists in similar fashion" 185.3. It will now be evident that what Dr. Stephen believes is suppressed is not indeed real guilt, but neither is it pseudo-guilt, it is neurotic guilt, a painful sense of guilt *unfitted* to the patient's real situation.

All this about guilt has been said without taking sides on the philosophical question whether guilt is a (i) non-natural and (ii) peculiarly ethical attribute. I believe that in the quotation above from 185.3 he wishes to say that it must be a more serious matter for a person who believes that guilt is non-natural and peculiarly ethical to suppress a feeling of guilt than for a person who believes that guilt is naturalistic and not peculiarly ethical. He has no evidence to give in favour of this, and equally I have none against it.

3.—*Religious Therapy and Psychoanalytic Therapy.*

At the beginning of section V, on religion, Mr. Lewis speaks of "the attempt to *dismiss* guilt as some matter calling for *mere therapeutic treatment*" as having been most stoutly resisted by religious thinkers. I think he means that the attempt has been made by Freudian psychotherapists.

Yet later in describing what some religious people do, he tells of a process hardly if at all distinguishable from therapeutic treatment: "From this desolation, a kind of doom or death which is the penalty of sin, *we may escape* in part by (1) sharp awareness of guilt and confession, and by

(2) the thrusting of the evil nature of the surrender to self deep into our consciousness in condemnation and remorse ; and by (3) grace even more, the inward flow of reality begins again " 195.8. Of course there are mentioned here some features of the world not usually mentioned in an analysis, and it is meant that the person really has done the evil he claims to have done. Why will it not do to write, " Of this desolation *we may be cured* in part by sharp awareness, etc." ? A neurotic but innocent patient goes to the analyst to escape his feeling of desolation ; a normal but guilty man goes to the minister of religion to escape his feeling of desolation. In both cases the person consulted tries to enable the man to admit to himself what the situation really is, to see himself as he really has been. The analyst tries to get the innocent man to see that he has not done what troubles him and could hardly have avoided the wish to do it and that therefore he has no ground for his feeling of desolation and so in part to escape it. The minister of religion tries to get the guilty man to face the fact that he has deliberately done what troubles him, and that he has indeed good ground for his feeling of desolation, to regret it now and feel remorse for it, and so in part to escape his feeling of desolation.* Each is a treatment which may in part bring relief from suffering. To what differences between them does Mr. Lewis wish to point ? What he mentions are (a) *awareness* of the guilt, (b) *confession* of the guilt, (c) *condemnation* of the guilt, and (d) *remorse* for the guilt. Each of these would, of course, be out of place with the neurotic patient since he is in fact mistaken in supposing himself guilty.

What this brings out is once more that what the psychoanalyst appears to be chiefly concerned with is unjustified feelings of guilt, and that no evidence has been given that when a person really is guilty the analyst tries to dismiss the guilt or regard remorse as out of place. But both the psychoanalyst and the minister of religion are concerned to bring relief.

* Neither of these claims to be a description of all that happens. By no means.

4.—*Freewill : Mr. Lewis's argument :*

Mr. Lewis says that "no conduct is open to blame unless it is free" 186.1. As evidence he instances that "it is a fair rejoinder to an accusation to declare that we 'could not help' what we did," and that "blame seems out of place in art or knowledge just because there is not the requisite freedom" 189.9. His worry is that there may be no cases of the requisite freedom, and hence no cases of guilt, so that in blaming someone we would always be being unfair.

What freedom is requisite? Not just "freedom to carry out a purpose," 186.8, *once we have chosen to*, but "freedom to choose" 187.2. (He speaks of this as "an *absolute* freedom of choice" 187.8, "an *open* choice," "a choice between *genuinely* open alternatives" 188.1.)

What are we free to choose between? Not motives ("We cannot summon up motives at will" 187.5). And if not motives, then not intentions ("It seems odd to suppose that our conduct is open to blame in regard to our intentions, but not in the way our intentions are shaped by our motives" 187.4).

So what is required if we are ever to blame is "some conduct that is free in a way in which neither (a) motives nor (b) acts from certain motives are free" 187.7, "in other words a power to *act independently of our desires and characters*," "actions which could have been other than they were even although *everything else in the universe* had remained the same" 190.5. However it is not that "*any* action may be expected from any man at any time," but that "there should be some occasions on which *the flow of our conduct in accordance with character* is arrested by a contrary claim of duty" 190.2.

There is not time to examine the steps of this argument, and all I intend to do is to say something short about (1) the conclusion and (2) the first steps.

4.1 : *The conclusion :*

4.11 : *The dilemma is worse than Mr. Lewis supposes :*

It is not merely that indeterminism (which would make it sometimes reasonable to blame people) is difficult to believe in (190.5), but that indeterminism, just as much as

determinism, seems to make it unreasonable ever to blame anyone. Mr. Duncan-Jones puts the point well in "Freedom : an Illustrative Puzzle" Ar. Soc. Proc. 1938-9, p. 108 :—" If the particular decision that I make, within a certain range, is not determined, then my previous life might have had the precise character that it actually had and I might none the less have made a different decision : I am therefore not responsible for having made this precise decision. And so on. A fortiori, unrestricted indifference is inconsistent with responsibility.

" We therefore have a simple constructive dilemma. If determinism is true, people are not responsible, and if the liberty of indifference is true, people are not responsible ; but either determinism or the liberty of indifference is true ; therefore people are not responsible." Cf. Prof. Stebbing, *Philosophy and the Physicists*, ed. 1, p. 239 :—" Suppose my acts of choice are spontaneous, uncaused, in no sense springing from what I am. How then can *I* be said to be responsible ? " And cf. Prof. Broad, this volume p. 10.6 :—" Just in so far as our volitions, etc., are undetermined they seem to fall under the heading of *mere accidental events that happen to us*, yet it is required that they should be expressions of our permanent inner nature."

4.12 : *Mr. Lewis's final suggestion seems to be a case not of indeterminism but of determinism.*

It will have been noticed that Mr. Lewis suddenly scales down the requirement :—" All that is required is that there should be some occasions on which the flow of our conduct in accordance with character is arrested by a contrary claim of duty " 190.2. There is nothing here to suggest that even if the character and the claim of duty had remained the same the flow could have been other than arrested. It seems to be rather determinism than indeterminism : " the flow of *x* in accordance with *y* is arrested by *z*."

So far from helping to solve the difficulty, these points make it worse.

4.2 : *The first step :*

Mr. Lewis rightly says, " It is a fair rejoinder to an

accusation to declare that we ‘*could not help*’ what we did” 186.1. But as people nearly always do in discussing freewill, he emphasises this side only, to the exclusion of the fact that it is further a fair rejoinder to such an *excuse* to claim that we “*could very well have helped*” what we did. We find cases in which a person could very well have helped it just as much as cases in which he could not help it. I wish to focus attention on these cases.

First I must instance cases of *being unable to help it* :—

(i) “We are not really to blame for our ignorance of psychical research. We cannot help it because we have not time to read so many articles.”

(ii) “He could not help it. They forced it out of him. He was given the third degree.”

(iii) “You can’t blame him for saying anything that is in this so-called confession. They interrogated him ten hours a day for weeks on end. When that is done a man becomes different ; he may even begin to believe some of the things they repeat to him.”

(iv) “They secretly gave him some sort of drug, and he spoke and acted in a queer, dreamlike way. He was beyond helping what he said or did.”

(v) “He just can’t help taking things. The truth is he is a kleptomaniac. You know he has everything that money can buy, and he does not even want the things he takes. He gets completely miserable about the trouble it brings, and tries hard not to do it again, but it seems he just can’t resist it. ‘It just comes all over me,’ he says.”

Now follow cases of *being very well able to help it* :—

(i) “Couldn’t help it? He could very well have helped it. He had received very good treatment at their hands and appeared perfectly normal in the witness box. He did not *have* to make that elaborate confession.”

(ii) “He’ll take anything he can lay his hands on, if he thinks he can get away with it. He has had every chance in life. He is just plain extravagant, a spendthrift.”

(iii) A boy comes home late from a party. His mother blames him saying, "You knew perfectly well you had to come away at 10 o'clock and it's nearly 12." He replies, "I couldn't help it. They just kept on doing things and I didn't get a chance to come away." She says, "Didn't get a chance! There was nothing to prevent you from just getting up at 10 o'clock and excusing yourself." Boy: "Yes, but you know how difficult it is when they're wanting you to do things." Mother: "I know, you've just got to try." Boy: "But I did try, Mother, only I couldn't." Mother: "Well you've just got to try harder. It's no use telling me you couldn't help it. You could very well. You've just got to learn to come away from things." Boy: "But the flow of my actions in accordance with my character just didn't happen to be interrupted by a claim of duty."

These examples indicate some of the things we are trying to find out in trying to find out whether a person could have helped doing what he did, and hence in trying to find out whether he is to blame for doing it. "No conduct is open to blame unless it is free" and these indicate what we are in fact concerned with in trying to find out whether his conduct was free. There was no question that there are cases of *necessity*. Is there any question that there are cases of *freedom*?

Asked by a man whether the material world exists I hand him a stone. Does that resolve his doubt (and mine)? No, but it may help us to begin.

